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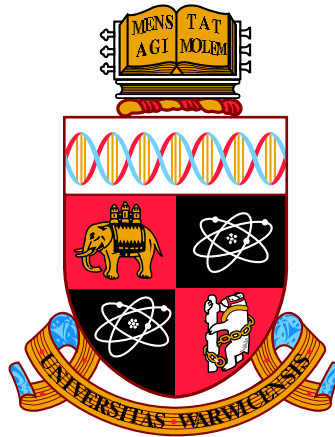
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# Bounded Rationality and Valuation

by

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**Thesis**

Submitted to the University of Warwick

for the degree of

**Doctor of Philosophy**

**Warwick Business School**

September 2018



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# Acknowledgments

I would like to thank my supervisors, Graham Loomes and Andrea Isoni, for all their time and advice that shaped this thesis. Throughout my annual reviews, Zvi Safra, Tigran Melkonyan, Jerker Denrell, Daniel Read, and Ivo Vlaev gave me invaluable advice to refine both the research questions as well as the methods in this thesis. I would also like to thank Ty Hayes, John Taylor, Evan Fradkin for help with setting up and analysing my experiment.

Also, I would like to thank my girlfriend, my family, and my friends for all their support throughout the years. Without them, I would have never been able to pursue this project.

Funding was received from Warwick Business School as part of the Network of Integrated Behavioural Science (NIBS) project. This work was supported by the Economic and Social Research Council Grant ES/K002201/1.



# Declarations

This thesis is submitted to the University of Warwick in support of my application for the degree of Doctor of Philosophy. It has been composed by myself and has not been submitted in any previous application for any degree.

The work presented (including data generation and data analysis) was carried out by the author. Substantial advice on the literature review, research questions, experimental design, analyses, and conclusions was given by the doctoral supervisors Graham Loomes and Andrea Isoni.

# Abstract

A computational valuation model is developed to predict discrepancies between choices and valuations in economic experiments. The model is based on Boundedly Rational Expected Utility Theory (Navarro-Martinez et al., 2017) and predicts average certainty equivalents for monetary lotteries that are higher than choices would imply from the same set of underlying preferences. Thereby, the model predicts the preference reversal phenomenon (Slovic and Lichtenstein, 1968).

The model predicts that a choice between a lottery and a sure payoff can influence a subsequent money valuation of the choice's strength of preference. This monetary strength of preference (MSoP), can be positively affected by spill-over effects from the choice process and also by consistency-seeking behaviour towards information about the choice. This can explain observations by Butler et al. (2014a) that participants systematically state MSoP values that are too high relative to their choices. Model simulations show how this model differs from existing computational valuation models. When adapted to predict MSoP values, these instead predict a negative MSoP mismatch.

These predictions are tested in a laboratory experiment, which finds a positive MSoP mismatch but only when MSoP values stem from upward adjustments to a sure amount. When sure amounts are adjusted downwards, a negative MSoP mismatch occurs instead. Neither the novel model nor existing theory can explain this two-fold pattern. Controlling for the delay between choices and MSoP valuations also rules out the possibility of spill-over effects.

Participants also value lotteries too high relative to their choice behaviour. Contrary to theory, a reaction time analysis shows that individually-longer reaction times do not reduce discrepancies between valuations and choice data. Preference reversals do not become less frequent when participants deliberate for longer.

Altogether, these results show novel and yet unexplained phenomena in valuation behaviour but also highlight how theory needs to be adapted to explain these.

# Nomenclature

\$-Bet	high-risk, high-outcome monetary lottery
BDM	Becker-DeGroot-Marschak (Becker et al., 1964)
BREUT	boundedly rational expected utility theory
CE	certainty equivalent
DM	decision maker
DV Task	direct valuation task
EUT	expected utility theory
IA Task	immediate adjustment task
LA Task	later adjustment task
MSoP	monetary strength of preference
P-Bet	low-risk, low-outcome monetary lottery
RP model	random preference model
SI point	stochastic indifference point
SI Task	stochastic indifference point task
SoP	strength of preference
SP model	stochastic pricing model

SVM mechanism	sequential value matching mechanism
WTA	willingness to accept a selling price for the right to play a lottery
WTP	willingness to pay for playing a lottery

# Chapter 1

## Introduction

Economic theory largely describes behaviour by assuming that people behave in a “rational” way. This implies that people can be described as decision makers who behave according to a clearly defined set of preferences. Actions that are inconsistent with these overarching determinants of behaviour are mistakes that are random or disappear with experience. A core component of these preferences is an attitude towards risk. Economic theory assumes that people differ in their tolerance to risk and that they trade off between options while taking the associated risk into account.

As any model, this simplifies the description of behaviour. But the question is if it is not too simple. Starmer (2000) summarises an abundance of behavioural regularities that are inconsistent with classic economic theory. Participants in economic experiments violate key assumptions of economic theory across a range of choice tasks and their choices correlate with aspects that are not considered by economic theory. They also vary in their behaviour across repetitions of the same choice problems. New theoretical approaches addressed this with the concept of “bounded rationality”, where individuals are assumed to be constrained in their ability to make optimal decisions. This led to the development of models that also incorporated effects on decision confidence, task difficulty, and the extent of individual deliberation in choices (Busemeyer and Johnson, 2004).

But participants also act inconsistently across elicitation procedures with well-documented discrepancies between choices and equivalence judgements, e.g.

valuation tasks. There is only limited research from the perspective of bounded rationality towards this and some predictions from theory have not yet been tested.

This thesis reviews existing research in chapter 2 and simulates existing boundedly rational valuation models in chapter 3 to provide a picture of current predictions from existing theory.

Chapter 4 develops a new approach towards modelling such choice-matching discrepancies. Boundedly Rational Expected Utility Theory (Navarro-Martinez et al., 2017) is extended to also capture valuations and generate predictions for phenomena that are not yet addressed by theory. This includes the quantification of strength of preference judgements in money units and how they can be affected by previous choices. Choices are modelled by a sequential sampling model, which accumulates mental evidence in the form of certainty equivalents. These are generated by a random distribution according to parameters of a random preference model (Loomes and Sugden, 1995). This mental evidence is also accumulated for a valuation process, which can be affected both by mental evidence from previous choices as well as choice displays of past choices. The model thereby not only captures existing behavioural regularities but also predicts effects that have not yet been tested.

Chapter 5 reports an experiment that is set up to benchmark different elicitation procedures against each other and to test predictions of relevant models. The experiment replicates previously observed effects and finds novel phenomena which cannot be explained by current theory. Binary choices, valuations, as well as valuations through adjustments of sure payoffs are elicited on a within-individual basis. This allows us to test to what extent the relevant models do predict effects of choice-matching discrepancies in the right direction and how different elicitation methods can generate conflicting measurements of preference.

Chapter 6 further tests for the validity of model properties: A reaction time analysis shows that the frequency of mismatches between direct valuations and choices does not decrease for individually-longer deliberation times of participants. This challenges an assumption from bounded rationality that decision makers approach optimal solutions with more deliberation. Specifically, all the sequential

sampling models discussed in this thesis predict that individually-longer deliberation times coincide with a lower rate of mismatches. This is because they all assume that with longer deliberation time, elicited valuations converge onto the prediction of a “core model”, which results in the same certainty equivalent that can be derived from choices. Evidence that this convergence does not exist suggests that valuation tasks cannot be described by such a sequential sampling process. Chapter 7 provides a conclusion and discusses opportunities for future research.

## Chapter 2

# Literature Review

This chapter provides the motivation for the research questions of this thesis. Section 2.1 explains why discrepancies between choice data and matching data poses a challenge for economic theory and section 2.2 shows how the same applies to elicitations of strength of preference in choices. The following sections then explain how economic theory has developed models that address these discrepancies (section 2.3), probabilistic decision behaviour in economic experiments (section 2.4), and how the concept of bounded rationality can be applied to explain behavioural regularities in choices (section 2.5.1) and valuations (section 2.5.2). Section 2.6 discusses the current state of the literature and identifies opportunities for the research in this thesis.

### 2.1 Choice-Matching Discrepancies

Tversky, Sattath and Slovic (1988) review several examples where participants' preferences in economic experiments systematically depend on the elicitation method. They categorise elicitation through choice and through matching. Choices are elicited by picking a preferred option out of two or more offered alternatives, comparable to a judgement that identifies the best option. E.g., whether a 25% chance of winning £50 is more attractive than receiving £10 with certainty. Matching means that participants set a value to be equally as attractive as an offered alternative



to them. This is comparable to an equivalence judgement where the attractiveness of an alternative is quantified. E.g., a valuation: What money amount is equally as attractive as a 25% chance of winning £50? Or probability matching: Which probability of winning £50 (in percent), is equally as attractive as receiving £10 with certainty?

Across a variety of experimental tasks, Tversky et al. (1988) show how elicited preferences can be inconsistent across procedures. I.e., participants systematically choose options that contradict their matched values. This is not just due to random variance of elicited preferences but instead caused by the type of procedure.

One of the examples given is developed by Slovic and Lichtenstein (1968; 1971; 1983) in the form of the preference reversal phenomenon. Slovic and Lichtenstein (1968) first generated evidence of this through the following experiment: Participants were offered to choose between lotteries with varying monetary outcomes as well as varying outcome probabilities. Outcomes were in the form of “winning” with positive payoffs and “losing” with negative payoffs. In another treatment, participants were asked for bidding prices for these lotteries. Whereas choices between lotteries were primarily influenced by the probabilities of a winning or losing payoff, bidding prices for the same lotteries were primarily influenced by the payoff amounts. Using this phenomenon, Lichtenstein and Slovic (1971) presented choice pairs of low-risk low-outcome lotteries, called P-Bets, and high-risk high-outcome lotteries, called \$-Bets, to participants. They then also asked participants for bidding prices for these lotteries. For a number of pairs, participants consistently chose the low-risk lottery over the high-risk lottery while also stating higher bidding prices for the high-risk lottery.

In a follow-up of this experiment, Grether and Plott (1979) controlled for a number of criticisms of the experimental method and incentivised participants. In two separate treatments, participants chose between various P-Bets and \$-Bets. They were then asked either for their selling prices of money or “the exact dollar amount such that you are indifferent between the bet and the amount of money” for both lotteries. 56% of participants chose P-Bets more frequently than \$-Bets *but*

*also* more frequently stated lower selling prices for P-Bets than for \$-Bets, thereby reversing their elicited preference. Only 11% showed a reversal in the opposite direction. Therefore, participants consistently reversed their preference from preferring the P-Bet in choices to preferring the \$-Bet in selling prices. This shows that participants systematically chose in a manner that was inconsistent with their valuation. This implies that at least one of the elicitation procedures failed to identify a common underlying preference.

This thesis will investigate choice-matching discrepancies that consider preference reversals between data from binary choices and valuations through a certainty equivalent (CE). A CE is provided when participants are asked for a money amount that they perceive to be *equally as attractive* as playing out a lottery. Also, this thesis only considers elicited preferences towards monetary lotteries and sure pay-offs that are immediate. E.g., “intertemporal” preference reversals as observed by Tversky et al. (1990) as well as observed reversals in “probability equivalents” by Hershey and Schoemaker (1985) would go beyond the scope of this thesis. In addition, repeated lottery choices and valuations with feedback about outcomes from playing out lotteries are not considered either. E.g., interesting results have been generated by Cox and Grether (1996), who found that repeated lottery valuations in an auction with feedback reduced the number of classic preference reversals. With more repetitions, \$-Bet valuations decrease and result in more consistent preference relations. But in a comparable experiment, Braga et al. (2009) find that this is caused by participants experiencing losses. Some participants with loss experience even reduced prices to an extent that results in preference reversals in the opposite direction. Also, participants who merely observe losses of others do not show the same reduction. This suggests that learning in this context does not eliminate the preference reversal and is interrelated with experience as opposed to straightforward learning from displayed information. Although this appears worthy of further investigation, incorporating feedback effects is beyond the scope of this thesis.

A preference reversal will be referred to as the phenomenon of a DM or participant choosing a monetary lottery A over another lottery B while independently

valuing B higher than A on different occasions. Therefore, the preference reversal is a special case within choice-matching discrepancies in general. If this occurs systematically, the individual reverses her preference relationship from choices compared to the preference from valuations.

In a review of Grether and Plott's (1979) and subsequent studies, Tversky et al. (1990) concluded that elicited preference was not independent of the procedure but instead context-dependent. Therefore, different preference relationships can be elicited with different procedures from the same participants: Choices result in a higher preference for P-Bets and valuations result in a higher preference for \$-Bets. In a further literature survey, Seidl (2002) narrowed down explanations of this procedure invariance to three possible causes<sup>1</sup>: 1) The elicitation mode of valuations, 2) intransitive preferences, and 3) over- and under-pricing of lotteries.

### **Elicitation Mode of Valuations**

Elicitation mode means that participants in preference reversal experiments might not respond with their true valuations. To understand this, it is necessary to understand the valuation procedure. Note that the preference reversal is a mismatch between choices and valuations. In the experiments discussed by Seidl (2002), lotteries were valued either by the participants' *willingness to pay* for playing the lottery (WTP), *willingness to accept* a selling price for the right to play the lottery (WTA), or the *certainty equivalent* (CE). According to classic economic theory, differences between all of these should be negligible if no transaction costs exist and income effects are negligible (Horowitz and McConnell, 2002). But Seidl (2002) lists a number of examples where WTP values were significantly smaller than WTA values, with CE values in between (also see Horowitz, 2002, for a review of WTA/WTP studies). If the regularity of too high WTA values is stronger for the \$-Bet, \$-Bets might be valued higher than P-Bets not because of participants' preferences but only because of the tendency to elicit too high WTA values. The same applies to

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<sup>1</sup>Seidl (2002) also elaborates on research concerning intertemporal preference reversals. These will not be discussed because this thesis only considers the classic preference reversal with immediate payoffs.

eliciting possibly lower WTP values for the P-Bet.

Testing for this in an experiment, Casey (1991) produced preference reversals in the opposite direction. But this only occurred for WTP valuations of bets with especially large expected values where the bidding process could lead to a loss for participants. The classic preference reversal remained even for WTP valuations of the P-Bet when any of these conditions was not met.

In addition, Karni and Safra (1987) and Segal (1988) proposed that participants in experiments might not be properly incentivised to report their actual WTA, WTP, or CE. All experiments used the Becker-DeGroot-Marschak (BDM) elicitation scheme (Becker et al., 1964). Participants that do not follow all decision rules of classic economic theory might then over- or underreport their valuations (see Seidl, 2002, for a detailed explanation). But Tversky et al. (1990) showed that the results also hold when using an ordinal payoff scheme, where participants are informed that their lottery valuations are only used to order their preference. If a lottery would be played out based on their valuation, the lottery with the highest assigned value would be played out. Therefore, the problem of misincentivisation disappeared but the preference reversal phenomenon remained.

### **Intransitive Preferences**

As part of developing a novel economic theory for choices under risk (regret theory, described in section 2.3), Loomes and Sugden (1982) and Loomes et al. (1989) propose that participants's preferences might not be entirely transitive. E.g., while the P-Bet would be preferred over the \$-Bet ( $P \succ \$$ ) and the \$-Bet over some sure amount ( $\$ \succ C$ ), these two preference elicitation need not imply a preference for the P-Bet over the sure amount. So these two preference elicitation do not mean that the P-Bet is also preferred over the sure amount ( $P \succ C$  does not follow). The same sure amount  $C$  might be preferred over the P-Bet ( $C \succ P$ ). And because a preference through valuation is elicited in a preference reversal experiment, higher \$-Bet  $CEs$  than P-Bet  $CEs$  ( $CE_{\$} > CE_P$ ) do not imply that the \$-Bet is preferred over the P-Bet ( $\$ \succ P$  does not follow).

Loomes et al. (1989) do find evidence for intransitivity in an experiment where participants choose over a range of the described  $P \succ/ \succ C \succ/ \succ \$$  choice tasks. But in a different experimental design, Tversky et al. (1990) find that only 10% of observed preference reversals can be explained by this type of intransitivity. They attribute the remaining 90% of preference reversals to over- and underpricing, which is explained in the following paragraph.

### Over- and Underpricing

In their experiment, Tversky et al. (1990) find patterns within preference reversals with the same approach of using a sure amount  $C$ , where  $CE_{\$} > C > CE_P$ . First, they identified preference reversals where  $P \succ \$$  and  $CE_{\$} > C > CE_P$  were elicited from participants. In a second step, they identified the preference relations between  $\$, P$ , and  $C$ :

- In 10% of cases, they observed a pattern of intransitivity where  $\$ \succ C$  and  $C \succ P$ .
- In 65.5% of cases, they observed overpricing of the \$-Bet where  $C \succ P$  and  $C \succ \$$ .
- In 6.1% of cases, they observed underpricing of the P-Bet where  $P \succ C$  and  $\$ \succ C$ .
- In the remaining 18.4% of cases, they observed both overpricing of the \$-Bet as well as underpricing of the P-Bet where  $P \succ C$  and  $C \succ \$$ .

This provides evidence for the over- and underpricing explanation (see sections 2.3 and 2.5 for theories that predict this). But Loomes and Pogrebna (2016) improve upon this method and infer specific sure amounts  $C_P$  and  $C_{\$}$  per participant and lottery, for which participants are *indifferent* between the lottery and the sure amount ( $C_P \sim P$  and  $C_{\$} \sim \$$ ). When eliciting CEs from these participants, it turns out that most participants report CEs that are higher than the specific sure amounts for *both* lotteries ( $CE_P > C_P$  and  $CE_{\$} > C_{\$}$ ). Therefore, *both* lotteries are overvalued by participants. Their experiment still replicates the preference reversal.

And the asymmetry observed by Grether and Plott of more participants choosing the P-Bet but valuing the S-Bet higher than vice versa also remains. But it shows that this occurs because the \$-Bet is overvalued even more than the P-Bet ( $|CE_{\$} - C_{\$}| > |CE_P - C_P|$  so that  $CE_{\$} > CE_P > C_P > C_{\$}$ ).

Since mis-pricing cannot occur in a choice task, overvaluation still seems to be a result of procedure invariance. But it still remains unclear if this overvaluation regularly occurs for all valuations. Schkade and Johnson (1989) find some evidence that mis-pricing can be influenced in an experiment where participants adjust pre-set starting values to arrive at a valuation for the P-Bet and the \$-Bet. When a starting value was especially high, lottery valuations ended up higher as well and vice versa for low starting values. Using this, Schkade and Johnson (1989) were able to modulate the rate of preference reversals up from 70% (for a low P-Bet starting value and a high \$-Bet starting value) down to 34% (for a high P-Bet starting value and a low \$-Bet starting value). See sections 2.3 and 2.5 for theories that incorporate such anchoring effects to explain over- and underpricing.

In conclusion, choice-matching discrepancies in the form of the preference reversal cannot be easily explained by a misincentivisation in the valuation process. Intransitive preferences seem to be unable to fully explain observations of follow-up experiments. But overpricing of lotteries in the valuation parts of the experiments can explain the preference reversal, although it is unclear how this is caused. The implication for a model that captures these characteristics is that procedure-invariance is expressed through different ways of eliciting preferences in choice and valuation tasks. Sections 2.3 and 2.5 give an overview of theoretical work in that regard.

## 2.2 Strength of Preference

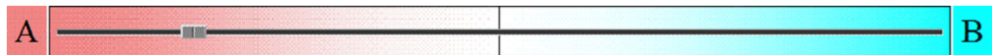
### 2.2.1 Eliciting Strength of Preference

The preference reversal is an example of procedure-invariance that provokes the question if other behavioural regularities can also interfere with the identification of

preferences. Comparing CE values of different lotteries can quantify the difference in preference between the lotteries. But choices only provide an ordinal preference in that instance without quantifying how *strong* this preference was. At worst, a choice in an experiment was made by a participant who actually was indifferent between options. This choice then carries the same weight in an analysis as all other choices by the participant. Also eliciting the strength of preference (SoP) would inform which preference relations were more difficult to discern for the participant.

Butler et al. (2014b) developed a simple measure of the strength of preference where participant first choose between lotteries A and B and afterwards report the strength of their preference. This is done by moving a slider on a scale that lists A and B at either end (see figure 2.1). The closer the slider is to the chosen option, the stronger the preference is. When the slider moved along the scale, slider positions were also accompanied with a text underneath that read “You think A (B) is BETTER” (depending on the slider being closer to A or B). Depending on the slider position the description would change from “SLIGHTLY better” to “BETTER”, “MUCH better” up to “VERY MUCH better”.

Figure 2.1: From Butler et al. (2014b): The ‘strength of preference’ instrument



Butler et al. (2014b) document that SoP responses vary across the entire range of the scale and that higher SoP values coincide with higher differences in the expected values of the lottery options. This occurs not only at the level of the participant but also across participants, which shows that SoP elicitation can be informative of a participant’s momentary preference in the context of a choice. Especially, it might show how SoP values might differ when participants do not make the same lottery choices across repetitions of the same choice problem. Still, Butler et al. (2014b) especially note two issues with their instrument: 1) A lack of

incentivisation and 2) limited comparability across participants. Issue 1) requires research to only rely on intrinsic motivation of participants to accurately report their SoP. Issue 2) makes it difficult to measure effects of variations in SoP values. One participant might regard the same point on the scale as “slightly better” while another would judge it as “very much better”.

Butler et al. (2014a) addressed these issues by eliciting (among other measurements) participants’ strength of preference in units of money. This monetary strength of preference (MSoP) was measured in how much all payoffs of the less preferred option would need to be increased in order to make the participant indifferent between options. Thereby, units of money made the MSoP more comparable across participants and the MSoP valuation task was also incentivised by using a BDM mechanism. Figure 2.2 shows a task display after a choice for lottery A (£10 with 50% and £8 with 50%) over lottery B (£11 with 50% and £0 with 50%). Clicking the “UP” button then increases both payoffs of lottery B by £0.10 for the MSoP. Within the task, participants could also subsequently reduce the MSoP down until the rejected lottery was displayed again as a minimum.

Figure 2.2: From Butler et al. (2014a): The ‘monetary strength of preference’ instrument (after a choice for lottery A)

**MONEY EQUIVALENCE TASK.** Use the 'UP' and 'DOWN' buttons next to lottery B (the one you did not choose) to adjust the lottery until you make it as good to you as lottery A:

**A**

1-50	51-100
<b>£10</b>	<b>£8</b>
50%	50%

Adjust:

UP  
DOWN

**B\***

1-50	51-100
<b>£11</b>	<b>£0</b>
50%	50%



Having a clearly quantified MSoP also allowed them to test if the measure gives values that are consistent with the participant’s choices. Later in the same session, participants were presented with binary choices between the option they had chosen earlier and the previously-rejected option with some predetermined amount added (e.g. £3) to both payoffs. If this amount was less than the MSoP sum stated for that pair, a participant should still have preferred the originally-chosen option if she had the ability to accurately estimate her MSoP. After all, she stated earlier that an improvement higher than this amount was required.

But instead, participants showed a tendency to reject the previously-chosen option in favour of the “enhanced” alternative, even when the size of the added amount was *less* than their previously-stated MSoP. This was taken as an indication that participants systematically overestimated their MSoP.

An MSoP mismatch will be referred to as the tendency of DMs or a participant to overstate their strength of preference in monetary units, their MSoP, between risky options. A trivial and correct MSoP of £1 would be to prefer receiving a sure amount of £1 over a payoff of zero. Systematically reporting an MSoP above £1 would constitute an MSoP mismatch. But the preference for a lottery over another option depends on an participant’s attitude towards risk and, as with the preference reversal, can be inconsistent with later actions.

As explained earlier, if participants are asked for a money amount that they perceive to be equally as attractive as playing out a lottery, this provides a CE. If an individual-specific CE can be assigned to any lottery, a participant acting consistently will prefer the lottery with the higher CE out of two lotteries. Additionally, the difference in CEs quantifies the difference in attractiveness to the participant.

This CE *difference* is a strength of preference in monetary terms. Participants who *only* state their MSoP between two lotteries without stating the respective CEs still respond to something logically similar to a valuation task. Instead of reporting two valuations of two options, they only report a single valuation difference between the options. Butler et al.’s (2014a) experiment shows that valuation differences in the form of MSoP values systematically mismatch with participants’

in choices in the same experiment.

In conclusion, measuring SoP and MSoP provides additional data on measuring preferences. But the MSoP mismatch calls into question if this data accurately reflects a participant's preferences. Although Butler et al.'s (2014a) experiment did not allow to test for causes of this mismatch, there exists a body of research on effects that might explain this MSoP mismatch. The following section gives an overview.

### **2.2.2 Choice-induced Changes in Reported Preference**

Butler et al. (2014a) already put forward a number of reasons for the MSoP mismatch. These can be assigned to three categories: 1) Attention to a subset of lottery characteristics, 2) an endowment effect, and 3) the reported preference being changed by the preceding choice.

#### **Attention to a Subset of Lottery characteristics**

Consider the choice problem displayed in figure 2.2, where a participant has just chosen a lottery paying either £10 or £8 with 50/50 probabilities. The rejected option is a lottery paying £11 with a probability of 50%. The participant now needs to add money to both payoffs, £11 and £0, until both lotteries seem equally attractive. Butler et al. (2014a) report that some participants in the experiment mentioned that they might have paid more attention to improving the first payoff, £11+ $x$  in this example. As they pay less attention to the other payoff £0+ $x$  and the probabilities of either payoff, they would underestimate how the adjustment increases the attractiveness of the rejected lottery. This could lead to eliciting too high adjustments because participants underestimate how strong the improvement actually is.

It is possible to circumvent this in an experiment with choices between a lottery and a sure amount. If only a single sure amount needs to be improved, participants only have one payoff and no probabilities to concentrate on. If the MSoP mismatch disappears in this design, attention to a subset of lottery characteristics

is likely to be a causal factor for the MSoP mismatch in Butler et al.’s (2014a) experiment.

### **Endowment Effect**

Having just made a choice for lottery  $A$  in figure 2.2’s example, it is also possible that participants assume ownership of lottery  $A$ . This could lead to an endowment effect, as proposed by Thaler (1980) and Kahneman et al. (1990) (see Knetsch, 1989; Morewedge, 2015, for an overview). Applied to this experiment, a participant would feel ownership of the chosen lottery  $A$  and perceive the MSoP task to be similar to a WTA elicitation for lottery  $A$ . Then, an endowment effect would entail a participant reporting a higher WTA than WTP for the lottery  $A$ . This is commonly known as the WTP/WTA gap. It could imply that participants adjust lottery  $B$  so that it is equally as attractive as their WTA for lottery  $A$ , with  $WTA > CE$  for lottery  $A$ . From this follows a too high MSoP that mismatches with their later choices. This hinges on the assumption that participants would report a MSoP between option  $A$  and  $B$  that would result in a lower MSoP if they had *not* just chosen an option.

This could be tested in an experiment where participants would also be prompted to reduce payoffs of a chosen lottery as well as CE for lotteries. If the pattern  $WTP \succ Lottery \succ WTA$  emerges in choices for the respective lotteries, the endowment effect would be an explanation for the MSoP mismatch.

### **Post-decision Changes in Reported Preference**

It would also be possible that the decision itself and not the MSoP procedure has an influence on the reported value. Izuma and Murayama (2004) review a number of experiments where participants show the tendency to rate or rank an option’s attractiveness higher after having chosen it. Systematically over a variety of contexts and elicitation methods, participants do show an increased preference for items or options they had chosen *if* they were aware of their choice. E.g., Sheth (1970) documented that housewives tended to rank personal care items higher after having chosen them.

It could be that participants trust their decision too much to represent their own preference. That way, they report a higher preference consistent with that decision because they overweigh the validity of the choice they just made. In a review, Moore and Healy (2008), classify this as overconfidence in the form of “Overprecision”. Moore and Healy define this as economic agents underestimating the variance of their belief across repetitions of the same task. E.g., when participants are prompted to give a confidence interval for the accuracy of their answers in a quiz, they systematically report too narrow confidence intervals. Applied to an MSoP task, this means that participants overestimate the “precision” of their belief. The consequence is that participants trust a single decision in the form of a choice they just made to be more precise of their average preference than it actually is. Therefore, they do not attenuate particularly strong feelings of preference when reporting their strength of preference.

In the case of judgement tasks with a subsequent elicitation of participants’ confidence in their decisions, Svenson et al. (2009) find that participants even distort factual information in favour of supporting the chosen alternative. Therefore, information search seems susceptible to a post-decision change as well.

So in the case of information search, overprecision does not explain this phenomenon because participants distort objective facts instead of confidence in their own judgement. But Festinger’s (1957) and Bem’s (1967) theories of cognitive dissonance offer an explanation (for a review, see Greenwald and Ronis, 1978). Cognitive dissonance means the mental state of participants when they consider evidence which is dissonant with their behaviour. E.g., considering why an alternative was unattractive while being aware that they have chosen it. Participants are assumed to dislike this state and therefore circumvent it by changing their attitude towards the alternative or even by altogether avoiding information that causes cognitive dissonance. Therefore, their information search might be affected by motivated reasoning: their reasoning may rely on a biased set of cognitive processes out of a motivation to arrive at the desired conclusion (Kunda, 1990; also see Jonas et al., 2001). Instead of deliberating in a way that generates their actual preference,

they deliberate in a way that supports their choice. If they had a momentary strong preference for an option, this might result in a subsequently overstated strength of preference after a choice.

Therefore in the Butler et al. (2014a) experiment, a post-decision change in preference could both be explained by the choice process having influenced the subsequent MSoP task (overprecision) as well as participants seeking an MSoP value consistent with their choice (cognitive dissonance).

There is also an existing approach in the modelling of judgement that could be applied to a post-decision change in reported preference: Two-stage dynamic signal detection theory (Pleskac and Busemeyer, 2010) provides a model how participants' deliberation during a judgement can influence a subsequent elicitation of a confidence measurement (Navajas et al., 2016, put this into perspective in the literature on eliciting confidence judgements).

The cognitive process is modelled as two consecutive stages: A choice stage and a confidence elicitation stage. A decision maker (DM) deliberates during a choice stage to come up with a judgement decision, based on a set of mental evidence (this is modelled through a drift-diffusion process, which is explained in more detail in section 2.5). This set of mental evidence is then carried over to the second stage, influencing the otherwise separate process for generating a confidence measure. That way, a spill-over of evidence from the choice to the subsequent confidence elicitation is possible.

Note that judgements, unlike preference elicitations, have correct and incorrect answers. Preferences between lotteries cannot be considered “wrong” but knowledge about the world can be, e.g. whether one of two displayed lines is longer than the other. Assume a judgement with options A and B, where a DM correctly chooses A for the majority of repetitions. Naturally, eliciting confidence into an A choice can only happen after the DM decided on A in the first stage. Therefore, a confidence elicitation into option A can only start off with a set of evidence that already led to choosing A. Since sets of evidence that would have led to choosing

B are left out, this means that the average confidence elicitation towards option A starts off with a set of evidence that favours A more than a priori. The result is overconfidence, where the DM reports a too high confidence level in the choice.

But since the second stage also adds a second set of evidence, it is nevertheless possible that some confidence elicitation are accurate or even revise a choice. Chen and Risen (2010) also describe a way in which this could apply to preferences, if the judgement is comparable to a choice and the confidence measure is comparable to an attractiveness rating. But while Pleskac and Busemeyer's (2010) approach can model overconfidence, it does not incorporate cognitive dissonance, where information is avoided.

Butler et al.'s (2014a) experiment shows that participants systematically quantify their MSOP in a way that is inconsistent with their choices on other occasions. Even though the literature offers several explanations for the MSOP mismatch, e.g. spill-over effects or motivated reasoning, it has not yet been tested which explanation can account for this phenomenon.

## **2.3 Expected Utility Theory and Other Deterministic Theories**

So far, this literature review has mostly covered empirical literature that shows how participants in economic experiments behave inconsistently. This and the next sections describe how economic theory has incorporated these and other behavioural regularities to fit into models of decision making and valuation. To keep the focus on possible explanations for the preference reversal phenomenon and the MSOP mismatch, this section will first concentrate on how classic economic theory is challenged by the preference reversal phenomenon and then concentrate on deterministic theories that can predict violations of procedure invariance. The following sections will then explain more advanced approaches.

Expected Utility Theory (EUT) still is the foundation of modelling preferences in most economic sub-disciplines. All economic agents, from consumers to

corporations, are assumed to be rational and expedient DMs in a plan to maximise their expected utility, given their initial position and knowledge of the world. For any alternative that a DM faces, e.g. a monetary lottery, the DM can assign a set of utilities correspondent to the states of the world in which they will happen. These states and their resulting utilities are assigned probabilities of being realised. Utility is usually derived from consumption of wealth and yields diminishing returns. This makes DMs sensitive to risk, i.e., potential variations in wealth. DMs are assumed to always want to consume more and to exhibit a certain level of risk aversion (or in some cases risk seeking instead) (Starmer, 2000).

EUT behaviour can be predicted by functions over utility that follow the standard axioms *completeness*, *transitivity*, *continuity*, and *independence* that safeguard the DM against “irrational” decisions (Savage, 1954; Luce and Suppes, 1965; Starmer, 2000; Sugden, 2004; Karni, 2008). Completeness implies that a preference exists for a DM between any two alternatives. Transitivity implies that preference orderings are consistent, e.g. if  $A \succ B$  and  $B \succ C$ , then  $A \succ C$ . And since any alternative is assigned a set of utilities with respective probabilities, this transitivity should also hold across elicitation procedures. Continuity implies that for any three alternatives  $A \succ B \succ C$ , a mix of alternatives  $pA + (1 - p)C$  with  $p \in [0; 1]$  exists, so that the DM is indifferent between  $B$  and the mix of alternatives. Independence implies independence of common consequences, i.e. that preferences are not affected by the possibility of outcomes that will happen independently of a choice. So if a DM prefers to choose lottery A over lottery B, the DM will also keep this preference if, with some probability, an irrelevant third lottery C is played out instead of the lottery from the choice (if  $A \succ B$ , then  $pA + (1 - p)C \succ pB + (1 - p)C$  for all  $p \in (0; 1]$ ).

This theory is not meant to be an entirely accurate reflection of human behaviour but instead creates a normative model, which individuals learn to follow on average. Individuals that act in a way that is inconsistent with their preferences, will keep themselves from maximising their expected utility. So individuals are assumed to strive for behaviour that follows EUT predictions. So in theory, deviations from

its predictions are either the result of a random error or insufficient learning. Out of their interest for utility maximisation, individuals will correct them in the future.

But the theory does not explain why human behaviour *systematically* violate EUT, which has been extensively shown in economic experiments. Starmer (2000) summarises findings of replicable human biases that conflict with an EUT prediction, notably violations of procedure invariance such as the preference reversal, but also ambiguity and loss aversion, violations of description invariance, transitivity, monotonicity, and independence. None of these can be accounted for by a simple error term. If these behavioural regularities reflect actual preferences, this might prove that humans behave “irrationally” and should learn to do better but conversely implies that a “rational” model cannot sufficiently explain their behaviour.

Tversky et al. (1988) developed an approach that can also describe the preference reversal phenomenon with Contingent Weighting Theory, which rests on two hypotheses on determinants of decisions: the prominence hypothesis and the compatibility hypothesis. Applied to the preference reversal, they treat the probability and payoff values as attributes that a DM assesses for choices and valuations<sup>2</sup>. As the P-Bet has an especially high winning probability and the \$-Bet an especially high winning amount, the most important attributes are probability for the P-Bet and payoff for the \$-Bet. Therefore, unlike in EUT, lotteries are compared not via the product of their attributes. Instead, the preference is constructed with sensitivity to the elicitation procedure and by assigning different levels of importance to different attributes.

In this case, the prominence hypothesis postulates that choices depend more on comparing the most prominent feature of lotteries. In the case of choice, the probability of winning is the primary attribute and the winning payoff is the secondary attribute. Therefore, the P-Bet is chosen more whereas P-Bets are valued higher than \$-Bets on fewer occasions, resulting in the preference reversal phenomenon. Continuing this logic, DMs exhibit more risk averse behaviour in choices than in valuations because they place a higher importance on the probability of winning in

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<sup>2</sup>Tversky et al. (1988) do not limit this to monetary lotteries but also discuss more general decision problems.



choices.

The compatibility hypothesis postulates that DMs base their decisions primarily on lottery attributes that are “compatible” with the elicitation procedure. Choices are more compatible with an accept/reject decision while valuations are more compatible with estimating a money amount. Because of this, using the winning probability as an “input component” is more compatible with choice as an “output”. Conversely, the winning payoff as an “input component” is more compatible with valuation as an “output”. Therefore, elicitations place a heavier weight on winning probability for choices and on winning payoff for valuations. This then leads to more preference relations in choices for the P-bet and fewer preference relations in valuations for the P-bet.

An additional approach along the lines of the compatibility hypothesis is Anchoring and Adjustment. Tversky and Kahneman (1974) proposed that a DM starts off a lottery valuation with a starting value, “an anchor”, which is then adjusted downwards. This adjustment is insufficient in reaching a valuation, that is equal to the DM’s CE in choices. Since the starting value is placed along the range of the lottery (going from the losing payoff until the winning payoff), this range is larger for the \$-Bet than for the P-Bet. If the starting value is placed close to the top of the range, it leads to \$-Bet starting values being larger than P-Bet starting values. This difference is then not entirely eliminated because the adjustment process is incomplete. The result is that \$-Bet valuations are frequently higher than P-Bet valuations.

All of these approaches use failure of procedure invariance to explain the preference reversal phenomenon, assuming that DMs construct their reported preferences differently in response to a choice or valuation task. Another approach relies on a choice rule that is the same across procedures. Regret Theory, proposed by Bell (1982), Fishburn (1982), and Loomes and Sugden (1982) in separate papers, models choices via comparisons of the lotteries’ possible consequences. Any lottery’s payoffs have a utility that also depends on the payoff of the *rejected* lottery. DMs anticipate this “regret” or “rejoice” in the possible states of the lottery. If a lottery

wins in a particular state of the world, it might still lead to regret if the *rejected* lottery wins more in that case (and vice versa with rejoice for avoided losses).

This can lead to series of choices that violate transitivity, which do occur in experiments (Loomes et al., 1991;). And these violations of transitivity can also explain the preference reversal without violating procedure invariance: For some parameters, a DM prefers a P-Bet over a \$-Bet in a direct comparison ( $P \succ \$$ ). It is possible to violate transitivity by using a sure amount  $C$ , where  $P \succ \$$ ,  $\$ \succ C$ , but also  $C \succ P$ . Therefore, the same DM is indifferent between the P-Bet and a sure amount  $P \sim CE_P < C$  as well as between the \$-Bet and a higher sure amount  $\$ \sim CE_\$ > C$ . This produces intransitive preference relations  $P \succ \$ \sim CE_\$ > C > CE_P \sim P \succ \$$ , which result in the preference reversal phenomenon.

Sopher et al. (1993) criticise Loomes et al.’s (1991) method by suggesting that participants’ detected violations of transitivity could just be random errors (see next section). But before describing this argument, it is first necessary to distinguish between participants’ “deliberate” preference reversals and those that result from random errors instead.

Starmer (2000) and Seidl (2002) list a variety of models that can also account for the preference reversal phenomenon. However, this section only serves to give an intuition of the two most important deterministic approaches in predicting this phenomenon: procedure invariance and intransitivity. But even if these approaches perform well on a descriptive level, they do not incorporate that actual behaviour is probabilistic. If it is unclear, to what extent decisions are inherently random, a participant’s choice is not a reliable indicator of their preference. The following section gives an overview how this affects preference elicitations.

## 2.4 Probabilistic Preference Models

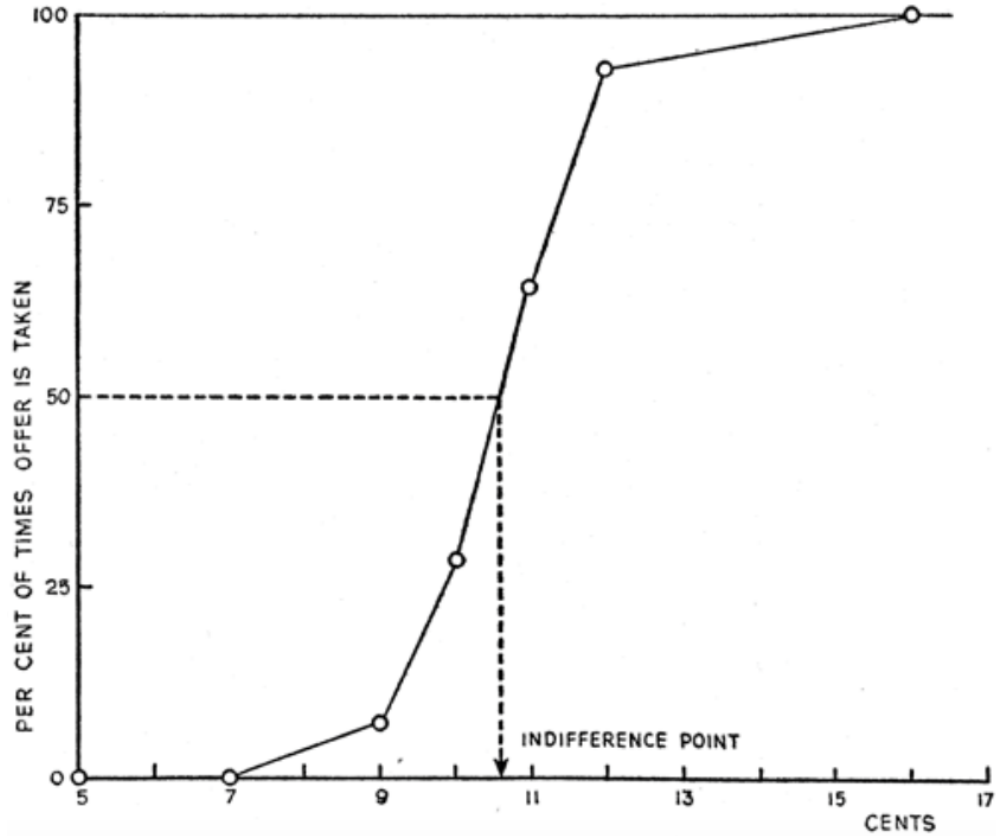
Mosteller and Noguee (1951) found that participants often show stochastic preferences when choosing between lotteries and sure amounts. Participants faced a choice between betting 5 U.S. cents on a lottery or keeping the 5 cents as a sure amount.

The lottery had a  $1/3$  chance of losing 5 cents and a  $2/3$  chance of a winning payoff that varied across trials. Participants would almost always choose the sure amount for low winning payoffs in repeated choices. But when the lottery's winning payoff was increased, participants gradually started to choose the lottery more often. Increasing the winning payoff even further eventually led participants to always choose the lottery as soon as its winning payoff was sufficiently high.

This gradual increase prevents the identification of a clear cut-off value that the participant perceives to be equally as attractive as the lottery. But instead it is possible to infer a stochastic indifference point (SI point). For each lottery, a theoretical lottery/sure amount pair will lead the participant to choose the lottery 50% of times, making the participant *stochastically* indifferent.

However, even when using a lottery and varying sure amounts, this SI point is not entirely equivalent to a reported CE as it can only be inferred. CEs can be directly elicited. Whereas an elicited CE is a response to a task, an SI point is a theoretical construct that can be used as a measure of preference. E.g., figure 2.3 shows how this was inferred for an exemplary participant in Mosteller and Nogee's (1951) experiment. The participant chose the lottery most times over sure amounts of 10 cents or less but rejected the lottery for sure amounts of 11 cents or more. Therefore, the SI point can be inferred to lie between 10 and 11 cents, where the participant is stochastically indifferent between the lottery and the SI point. Also, the participant does not have a deterministic transitive preference  $11\text{¢} \succ \text{Lottery} \succ 10\text{¢}$ . The participant's behaviour obeys *weak stochastic transitivity* (Block and Marschak, 1960), meaning that preference relations are transitive but only hold with a probability above 50% (if  $P[A \succ B] > 50\%$  and  $P[B \succ C] > 50\%$ , then  $P[A \succ C] > 50\%$ ).

Figure 2.3: An exemplary subject's repeated choice behaviour between a lottery various sure amounts offered. Source: Mosteller and Nogee (1951, figure 2)



This is a challenge for the deterministic theories described in the previous section because weak stochastic transitivity cannot stem from errors that are entirely random. The participant featured in figure 2.3 makes consistent choices between the lottery and sure amounts far from the SI point. But these choices become more and more random for sure amounts closer to the SI point. Note that in theory, the sure amount at a DM's SI point can also serve as a lottery CE. This is because the DM is on average indifferent between the lottery and the sure amount at the SI point.

The remainder of this section will deal with models that capture probabilistic behaviour and literature that relates probabilistic behaviour to the preference reversal. The modelling approaches that are described here are of the type commonly called Luce's choice rule, Fechner model, Tremble model, and Random Preference

model.

### **Luce’s Choice Rule**

Luce’s choice model is an early model that formalises probabilistic preferences by relating them to choice probabilities (McFadden, 1973). Luce (1959) described the likelihood of a DM’s lottery  $A$  choice through a “response strength”, which can be equated to its expected utility  $U(A)$  when applied to EUT. In this case, a choice probability for lottery  $A$  in a binary choice between lotteries  $A$  and  $B$  is equal to its utility to the power of a subjective parameter  $\gamma > 0$  divided by the sum of all utilities with the same exponent:  $P(A \succ B) = U(A)^\gamma / (U(A)^\gamma + U(B)^\gamma)$ . This way, a curve as in Mosteller and Nogee’s figure (2.3) can be predicted. But this approach can give some counterintuitive predictions, e.g. DMs often choosing a sure amount of £11 over a sure amount of £10. See Blavatskyy (2011) for an adaption of the model that addresses this.

### **Fechner Model**

Hey and Orme (1994) use a Fechner-type model, where a DM computes the difference in utility between options and the resulting difference is subject to a symmetric error term  $\varepsilon$ : The DM chooses lottery  $A$  if  $U(A) - U(B) + \varepsilon > 0$ . That way, the error term  $\varepsilon$  can randomly “overturn” a preference relation<sup>3</sup>. This becomes more likely for lotteries that are more similar in utility, also predicting increasingly probabilistic choices closer to an SI point as in figure 2.3.

### **Tremble Model**

Harless and Camerer (1994) develop a model that uses the “trembling hand” concept from game theory (Selten, 1975). Optimal choices are perfectly identified through EUT but randomly overturned with a fixed probability  $\tau \in [0; 0.5]$ . That way, a DM chooses lottery  $A$  with probability  $(1 - \tau)$  if  $U(A) > U(B)$ , regardless of the

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<sup>3</sup>If the utility difference plus the error term  $\varepsilon$  equals zero, a tie-breaker through a random choice is used.

difference in utility. I.e., choices follow deterministic preferences but sometimes the DM “pushes the wrong button” and does not act according to her actual preference.

### Random Preferences

So far, all models assume that probabilistic behaviour comes from extraneous errors, e.g. carelessness, fatigue, insufficient motivation, or calculation errors. Built on previous work by Becker, DeGroot, and Marschak (1963), another approach by Loomes and Sugden (1995), assumes that DMs do not possess a stable underlying preference. The random preference model (RP model) assumes a utility function where the risk aversion parameter randomly deviates across choices.

E.g. in a EUT approach, the utility of an 80% chance of winning £12 can be computed with the utility function  $u(x) = x^{1-r}$  with  $r$  as a fixed parameter for the degree of risk aversion. Then, this lottery’s utility would equal  $80\% \cdot (12)^{1-r}$  and could be compared with another lottery’s utility, always generating the same preference relation. The RP model uses a utility function where, for each choice, the degree of risk aversion  $r$  is independently drawn from the same given random distribution. Thereby, the randomness that leads to probabilistic preferences is built into the model without an error term<sup>4</sup>.

All of the models discussed in this section still are stochastically transitive. So the question remains how compatible noise explanations are with intransitivity in explaining the preference reversal phenomenon. Sophier and Gigliotti (1993) used a specification similar to the tremble model, where they assumed that a DM has transitive  $C \succ P \succ \$$  preference relations (with  $C$  as some sure amount). But each choice can be subject to an error with probability  $\tau \in (0; 0.5)$ . Then, recreating the preference reversal experiment in theory, the majority of choices yield the correct ordering  $C \succ P$ ,  $P \succ \$$ ,  $C \succ \$$ . Observing the choices  $C \succ P$  (correct),  $P \succ \$$  (correct),  $\$ \succ C$  (incorrect) can occur with one error. This erroneous preference relationship is the non-transitive ordering that results in the preference reversal.

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<sup>4</sup>The random preference model can also be combined with extraneous noise. E.g., as done by Loomes et al. (2002), Loomes (2005), and Bhatia and Loomes (2017).

But observing the choices  $P \succ C$  (incorrect),  $\$ \succ P$  (incorrect),  $C \succ \$$  (correct) only occurs with *two* errors. This is an erroneous non-transitive ordering of a P-to-\$ reversal, which is less likely to occur because it requires two errors instead of only one. And this “reverse” reversal was much less observed in the preference reversal experiment as well.

Sopher and Gigliotti then applied this logic to an experiment where they estimated the probabilities of making mistakes on questions of the  $C$ ,  $P$ ,  $\$$  type. They concluded that using when accounting for errors of the type described, a non-transitive model did not provide a better fit for the data.

But except for the tremble model, all models’ choice probabilities for the stochastically preferred option vary across choice problems. E.g., consider a lottery with an SI point of £10 with a DM that is risk averse (or mostly risk averse in the RP model). The DM will choose the lottery more often both for sure amounts £9 and £1 because both are below the SI point (still, not necessarily all of the times). But like in the Mosteller and Noguee data, the DM will choose £1 over the lottery *fewer* times than £9. If any sure amount choice below the SI point counts as an “error” in an experiment, this means that each choice problem has its own error rate.

Birnbaum and Gutierrez (2007) address this issue by identifying an individual-specific error rate for each choice among a set of lotteries<sup>5</sup>. In their experimental design, each of these choices is repeated to make an estimation of this error rate possible and to infer if observations of non-transitivity are consistent or random. Like Sopher and Gigliotti (1993), they concluded that stochastic transitivity was not consistently violated in their experiment.

Bostic et al. (1990) studied whether the preference reversal phenomenon also occurs when the preferences in choices is indirectly inferred from a participants’ binary choices. As a baseline, they replicated the preference reversal phenomenon using binary choices between and direct CE elicitations of lotteries with P-Bet and

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<sup>5</sup>Lotteries were taken from an experiment by Tversky (1969) to demonstrate intransitive preferences. In a follow-up, four more lotteries were added to test for an interaction effect between winning probability and payoff.

\$-Bet parameters similar to Slovic and Lichtenstein (1968).

In a second step, they used a different method to infer CEs of these lotteries. Instead of a direct CE through valuation, they inferred each lottery’s CE from choices between the lottery and various sure amounts. This was done by presenting binary choices between the respective lottery and a sure payoff, randomised to be at either end of the lottery’s range. If participants chose the lottery (sure amount), the sure amount was increased (decreased) in steps for a subsequent choice. When choices did not change the lottery-sure amount preference, the step size was decreased until the step size was 2 U.S. cents. The last displayed sure amount was then defined as the participant’s SI point, equal to a CE inferred from choices<sup>6</sup>.

They found that comparisons of these CEs in choices to CEs from direct lottery valuations decreased the share of preference reversals but did not remove the preference reversal phenomenon entirely. There was still a significant share of participants with a higher P-Bet CE in choices who nevertheless stated a higher \$-Bet CE in valuations. Loomes and Pogrebna (2016) also replicated this result when they inferred P-Bet and \$-Bet SI points by observing repeated choices of each lottery against a pre-determined set of sure amounts. Therefore, violations of stochastic transitivity within choices cannot be entirely eliminated but they are not the only force driving the preference reversal phenomenon either. Procedure invariance also remains as an explanation.

Another modelling approach also assumes that elicited CEs can be probabilistic. Following a proposed model by MacCrimmon and Smith (1986), Butler and Loomes (2007) develop a model that uses the concept of imprecision in preferences to predict the preference reversal phenomenon<sup>7</sup>. Consider an 80% chance of £12 P-Bet and a 25% chance of £50 \$-Bet. A DM will be able to estimate their CE for the lotteries to lie within these lotteries’ ranges. So DMs know that their CE is within the range (£0; £12) for the P-Bet and (£0; £50) for the \$-Bet. Butler and Loomes (2007) also define an “imprecision interval”, a subset of that range, where a

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<sup>6</sup>Specifically, Bostic et al. refer to this as a “choice indifference point”.

<sup>7</sup>The model can also predict preference reversals in probability equivalents, which is not discussed here.



DM cannot be sure whether a sure amount is greater or smaller than their CE. E.g., with a CE of £9 for the P-Bet the DM would find £8 and £10 hardly distinguishable in attractiveness from the P-Bet. But what if that imprecision interval is larger for the \$-Bet? Since the \$-Bet has a smaller winning probability but a larger payoff, the DM might find it even harder to distinguish an amount from the \$-Bet's CE. E.g., with a CE of £8 for the \$-Bet the DM could find £5 and £15 hard to distinguish. Note that the P-Bet's range ends at £12. And since the \$-Bet has a larger range than the P-Bet, there is more opportunity for imprecision to make \$-Bet valuations above £12 hard to distinguish. E.g., the extreme case of a £9 payoff with probability 100% would have the smallest possible interval of exactly £9.

So the \$-Bet offers “a lot of room” for high valuations while the P-Bet does not. And imprecision can then lead to a spread of \$-Bet valuations that encompasses all P-Bet valuations. Since \$-Bet valuations are likely to lie beyond the maximum payoff of the P-Bet but are, like the P-Bet, bounded to be above zero, median \$-Bet valuations can be higher than median P-Bet valuations. This can replicate the preference reversal without violations of stochastic transitivity in choices.

Butler and Loomes (2007) then estimated P-Bet and \$-Bet imprecision intervals for participants through incremental choices between the lotteries and sure amounts, starting both at the upper and lower end of the lottery range (they also elicited participants' confidence in these choices). When comparing imprecision intervals between the P-Bet and \$-Bet, they found that the imprecision intervals were able to explain the preference reversal phenomenon.

Collins and James (2015) further develop this approach by combining it with Blavatsky's generalisation of Luce's Choice Rule (briefly described in section 2.4). Using this, the choice-matching discrepancies arising through CEs vs. probability equivalents are explained by response mode as per Butler and Loomes (2007). And probabilistic choices are explained through Blavatsky's (2012) stochastic choice model. Then, the frequency of preference reversals can fit in with model predictions.

This shows how probabilistic models can offer a more realistic approach in describing behaviour because they predict the innate randomness in choices (and

valuations in case of Butler and Loomes, 2007). Section 3.2 in chapter 3 shows a possible MSoP elicitation in the RP model’s case. But they do not predict the preference reversal phenomenon of the sort shown by Bostic et al. (1990). The following section describes a new wave of models that use the concept of bounded rationality and are applicable to the preference reversal. This field offers new opportunities to model the interplay between choices, valuations, and MSoP values and to also make predictions about confidence in decisions and reaction times.

## 2.5 Bounded Rationality

The preference reversal phenomenon is only one among a breadth of behavioural regularities challenging the EUT assumption that behaviour can be modelled entirely through a well-defined set of stable preferences. Instead, participants might be unable to choose according to an exact utility over all possible events, given constraints in mental resources and time. But they could still decide in a way that is “good enough” to justify their investment of these resources. This strategy was defined as “Bounded Rationality” by Simon (1955) and reflects a key assumption of a new generation of cognitive models where this governs a process of decision making.

Concerning choice, this section will cover how two sequential sampling models, belonging to a subgroup of models that assume bounded rationality, can be used to generate predictions of lottery valuations. There is a variety of other sequential sampling models (for reviews see Busemeyer and Johnson, 2004; Ratcliff et al., 2001, 2016; Vlaev et al., 2011), such as the Competing Accumulator model (Usher and McClelland, 2001), the ECHO model (Guo and Holyoak, 2002; Glöckner, 2007), the drift-diffusion model with time-varying boundaries (Fudenberg et al., 2017), or the Decision by Sampling model (Stewart et al., 2006). Other models use bounded rationality without a mental sampling process, for instance Decision from Experience (Hertwig et al., 2004) where choices are based on previous learning, or Heuristic Decision Making (Gigerenzer and Gaissmaier, 2011) where DMs use simple choice

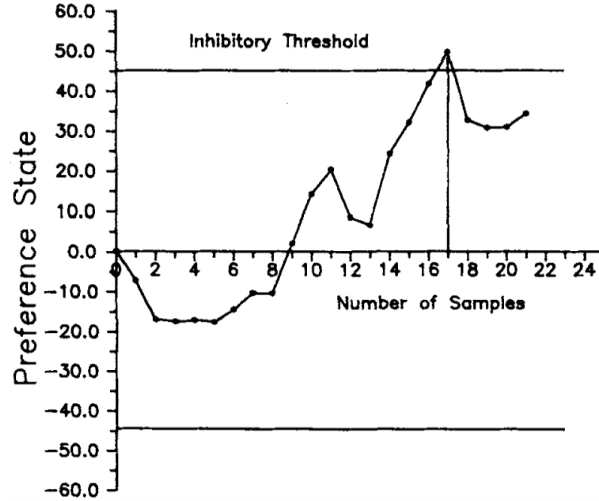
rules.

But this thesis focusses on bounded rationality applications to lottery valuation. So this section will only consider two choice models: 1) A sequential sampling model that can already predict lottery valuations, Decision Field Theory (Busemeyer and Townsend, 1993), and 2) Boundedly Rational Expected Utility Theory (Navarro-Martinez et al., 2017) because this choice model is adapted to predict valuations in chapter 4. The section on valuation models describes 1) Query Theory, a qualitative sampling model that explains the WTA/WTP gap through task-specific valuation processes 2) the Sequential Value Matching mechanism, a valuation model based on Decision Field Theory, and 3) the Stochastic Pricing Model (Blavatsky and Köhler, 2009b), a model with a boundedly rational valuation process relies on the RP model for choices.

### 2.5.1 Sequential Sampling Models

Vickers et al. (1971) developed a model where a mental process accumulates samples of “evidence”, a perceived magnitude which is subject to noise and highly correlated to the actual magnitude of a stimulus, to determine which of two stimuli is stronger. Throughout the process, the DM sequentially samples this evidence. As soon as the level of accumulated evidence through these samples passes beyond a exogenous threshold, the DM stops the process and decides in favour of the option that yielded more evidence (see figure 2.4). The more often samples of evidence are accumulated, the higher the likelihood of a correct choice becomes. This can be interpreted as an unobserved confidence dimension of the judgement. This likelihood of a correct choice, the choice’s accuracy, can be increased by a higher threshold when the DM is willing to invest a lot of resources in order to increase the sample’s validity, or decreased by a higher level of noise when it is hard to determine which objective magnitude is higher.

Figure 2.4: Vickers et al.’s (1971) sequential sampling process with an additional inhibitory threshold to model a confidence level. Source Busemeyer and Townsend (1993, figure 1)



(Note that the inhibitory threshold is crossed at sample 17, implying that a choice towards the favoured option is triggered after sampling 17 times.)

Vickers et al. (1971) only apply this to objective measures, in their case a time-recorded judgement of participants whether one of two visible blinking lamps has a higher frequency. If this is extended to an economic context, participants might decide inconsistently between options in repeated choice tasks as presented by Mosteller and Nogee (1951). Not out of mental constraints in connecting probabilities and payoffs or a preference for randomisation, but because they do not know with certainty which option is more attractive to them. Furthermore, since the process of eliciting a preference cannot be considered to have a “correct” outcome, it is impossible to detect a correct choice. Instead, only the propensity to choose in a particular choice problem can be inferred from observing participants’ behaviour. See Busemeyer and Johnson (2004) and Ratcliff et al. (2016) for overviews of relevant modelling approaches from the sequential sampling literature.

**Reaction Times in Choices** A fundamental assumption of sequential sampling is that there is a speed-accuracy trade-off. The accuracy lies in the the DM identifying a correct answer and the speed in how quickly the decision process is over. But

how can this be measured in an experiment on preferences? Speed can be inferred relatively easy by analysing if a given task is completed more quickly on an individual level. But measuring accuracy is a more challenging problem because it is hard to determine what “accurate” preferences are.

Moffatt (2005) estimated the best fitting risk attitudes for each participant in a binary choice experiment by Hey (2001) where reaction times were measured. Using these risk attitudes, Moffatt was able to predict “closeness to indifference” for the choice problems faced by each participant. I.e., how similar in attractiveness the two options were, based on the participant’s risk aversion.

Moffatt then analysed the effect of closeness to indifference on reaction time in a random effects regression. To improve the analysis, Moffatt also measured difference in the lotteries’ parameters as a proxy for “objective” dissimilarity, the number of outcomes in the lottery with fewer outcomes as measure of complexity, the trial numbers of the choice in the order of choice tasks and all experimental tasks as measures of task experience, and the logarithm of the expected payoff of the less complex lottery to measure the strength of financial motivation. Altogether, Moffatt used a number of factors that would incentivise a participant to consider a lottery choice more carefully, one of which was how close to indifference the participant was. Moffatt finds a highly significant positive effect of closeness to indifference on reaction times. This means that participants take longer, the closer to indifference they are between options. Moffatt suggests that this time is used to allocate more cognitive effort to the task: If lotteries are hard to distinguish in their attractiveness, the participant spends more time deliberating for the choice.

Similar results have been observed in participants’ choices for lotteries that also involve non-monetary consequences (Diederich and Busemeyer, 1999; Diederich, 2003), and for non-monetary items with increasing attribute similarity (Bhatia and Mullett, 2018). This shows that participants in a variety of choice experiments do find it difficult to identify their own preferences and endogenously trade off decision speed against accuracy. But the problem remains how to organise this in a formal model. And in a second step, how such a model could also predict choices and

valuations that capture the preference reversal phenomenon.

**Reaction Times in Valuations** A speed-accuracy trade-off might also be relevant for valuations. Schkade and Johnson (1989) recorded reaction times for valuation tasks of and direct choice tasks between P-Bet and \$-Bet type lotteries. While \$-Bet valuations took longer than P-Bet valuations, valuations generally took longer than choices.

But this cannot be easily interpreted as a valuation requiring more mental evidence. It might be that a participant just takes longer to process the same amount of mental evidence in a valuation task or that the execution of reporting a valuation takes longer than a choice. Still, some findings can be inferred. If we assume that the valuation *process* is the same for \$-Bets and P-Bets and takes longer for the \$-Bet, it follows that participants need to deliberate longer to report a valuation for a \$-Bet. This can be detected through the *marginal* effect of individually-longer reaction times. If participants on average take the same amount of time to detect the valuation task and to report their lottery valuation in the experiment, the remainder of the variation in reaction times can be attributed to the mental valuation process.

Therefore, participants take longer to value \$-Bet type lotteries because they find it more difficult. Schkade and Johnson (1989) also found evidence of this because the variance of \$-Bet valuations was larger than for P-Bet valuations. But it is unclear what effect individually-longer deliberation times might have on repetitions of valuations of the same lotteries.

**Testing for Reaction Time Effects** When accounting for individual and task-specific effects, any behaviour that is associated with a larger mental sample should also occur when longer deliberation times occur. E.g., if (everything else equal) a DM reports lower valuations for longer sampling times, this predicts that participants should also on average report lower valuations when deliberating longer than usual for the same lottery.

However, it is impossible to observe a participant's actual deliberation time. Any experimental procedure introduces noise and participants need additional time

for understanding the task as well as executing their choice (for an overview of the fundamentals of reaction time analysis, see Luce, 1986). At this point, it becomes important to distinguish between deliberation time and reaction time. Deliberation time is the unobservable amount of time that a participant needs to finish the *mental* process of deciding something without yet executing the decision. Experiments can only record indirect measures such as reaction time, the time in an experiment that passes from presentation of the task until the task is confirmed to be completed by the participant. Therefore, reaction time encompasses deliberation time and adds another layer of experimental noise over the unobserved deliberation time (also described as “non-decision time” by Ratcliffe and Tuerlinckx, 2002).

But it is still possible to limit predictions of reaction times to marginal effects. A marginal effect disregards the absolute value of a dependent variable and only represents the statistical effect of a one-unit change in the independent variable. E.g., instead of associating predictions to a set of parameters and a specific reaction time, we could limit the prediction to what happens for individually *longer* reaction times. Then, a test of this prediction is possible without the need to exactly estimate an unobservable factor such as deliberation time.

### 2.5.1.1 Decision Field Theory (DFT)

Bussemeyer and Townsend (1993) use Vickers et al.’s (1971) accumulator model to develop Decision Field Theory (DFT) in which the sequential sampling process stands for a DM who statistically samples her own preference relationship in face of a binary choice. Samples are the difference in subjective expected utility between the options and are subject to randomness as the DM is assumed to have fluctuating attention between the events that determine the payoffs. But the utility remains fixed and therefore the source of noise lies in the attention switching process (unlike in any of the probabilistic preference models in section 2.4). These “valence differences” between the subjective utilities of the two options are then repeatedly sampled in infinitesimally numerous and small intervals, thereby resulting in a dynamic process.

At the start of the process, the accumulated differences lie at zero. I.e., no

option has any evidence in its favour. But then the valence differences accumulate and result in a level of overall evidence, until a threshold of sufficient evidence is reached. Then, the DM takes action and chooses the option that was favoured in the process (similar to the perception task illustrated by figure 2.4). The threshold implies a level of confidence that is “good enough” for the DM to stop sampling and take a choice, given the mental constraints and the importance of the choice.

Since the DM’s attention does not consider the exact same lottery states across repetitions, valence differences fluctuate accordingly. And these valence differences are greater for states of the world where underlying utility differences are greater, i.e. when payoffs of a particular option are much higher. So, valence differences are subject to randomness but on average accumulate evidence in favour of the option with a higher underlying utility. Thereby, DFT predicts probabilistic choices as observed by Mosteller and Nogee (1951).

The amount of sampling that a DM needs until the threshold is reached can also generate predictions for reaction times. If the threshold is higher, the process takes longer because the DM has a higher required confidence. But the process can also take longer because valence differences are smaller. This is the case when options only have small differences in underlying utility. Therefore, the DM needs to sample for longer for options that are more similar in utility. This also predicts that choices take individually-longer when participants are closer to indifference between options, as observed by Moffatt (2005).

This process can also be refined to capture additional behavioural regularities. The process can be subject to other cognitive influences, for instance the starting point can be moved closer to either threshold to model a DM who is biased due to prior knowledge or past experience. But also the change rate of the accumulated valence differences can be altered over time to favour evidence early or late in the process, resulting in primacy and recency effects. In addition, a goal gradient<sup>8</sup> can hinder the speed of evidence accumulation in case of events that entail

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<sup>8</sup>Initially used in the psychological approach-avoidance theory, the goal gradient reflects the effect that DMs perceive positive or negative utility of an event as higher the more committed they are to an action (Epstein and Fenz, 1965). The goal gradient is used in DFT to have a stronger



losses. This results in loss aversion as it increases the probability of the accumulated evidence reverting back to a choice with no or smaller losses. Diederich (1997) extended DFT to also apply to multiple attributes where each accumulated sample switches from considering one attribute to another, following a Markov process<sup>9</sup>. Even reversals of choice preference between options as a result of time pressure are possible (Busemeyer and Diederich, 2002), which has been consistently observed in experiments (e.g. Edland and Svenson, 1993). Roe et al. (2001) further generalised DFT to address multiple choice alternatives as well.

DFT still uses the classical economic concept of a fixed utility but can predict probabilistic choices, while assuming that a DM can theoretically converge to her true preference without noise in the limit. So inconsistencies in actual choices do not reflect the DM's innate preferences but instead the noise of a preference elicitation process that can be reduced by investing more mental resources.

#### **2.5.1.2 Boundedly Rational Expected Utility Theory (BREUT)**

Navarro-Martinez et al. (2017) developed Boundedly Rational Expected Utility Theory (BREUT), a sequential sampling model with the RP model as a source of mental evidence. BREUT is explained in more detail in section 4.2, chapter 4, to demonstrate how a valuation model can be built through adapting BREUT. So this section only gives a short description to put it into context within the sequential sampling literature.

BREUT features a DM who sequentially samples evidence from an RP model to inform a choice between two alternatives. The RP model provides a CE for each option based on the same degree of risk aversion, but this risk aversion is sampled anew with each sampling step. The DM computes the CE difference between the two options for each of these steps. This evidence is accumulated by building a sample of CE differences between the two options through sequential steps. With each step, the mean of all the CE differences shows a temporary strength of preference for

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effect on perceived utility in case of a gain and a smaller effect in case of a loss.

<sup>9</sup>More precisely, the special case of an Ornstein-Uhlenbeck process that is mean-reverting with a long-term drift (Diederich, 1997).

either option.

The question is now at which sampling step the DM should stop the process and settle on an option. The DM trades off the accuracy of the sample against the speed of the decision. She prefers a larger sample as long as her required confidence in the sample's accuracy is not yet achieved. The decision rule for this is a simple t-test, with the required confidence as the corresponding p-value. As soon as the t-test shows that the mean of CE differences is sufficiently different from zero, the DM stops the process and settles on the choice with the higher CE.

To model constraints in time and attention, the DM is also assumed to reduce the required level of confidence with each step. So the longer the sampling process takes, the more willing the DM is to take a choice that would be elicited through the mean CE of all RP utility functions.

So like in DFT, BREUT uses a noisy source of mental evidence from which the DM samples. BREUT uses an endogenous stopping rule and can thereby, like DFT, predict reaction times with the sample size as a proxy. And while BREUT has a measure of mental evidence that seems easy to extend to a valuation process, the question remains what kind of valuation process would be best to explain a DM's behaviour.

### 2.5.2 Boundedly Rational Valuation models

To apply the same speed-accuracy trade-off from boundedly rational choice models to a lottery valuation task, it is necessary to adapt the DM's process from comparing two or more lotteries to valuing a single lottery. Instead of answering the question "*which* is better?", the DM needs to answer "*how good* is it?". Sequential sampling models answer the first question through a process of comparison between alternatives that stops as soon as the difference is meaningful. But to answer the second question, a valuation model will not only generate a single valuation and update it in a process but also determine when to stop this process without another value to compare it to.

The speed-accuracy trade-off in a valuation process bears some resemblance

to an exploration–exploitation dilemma<sup>10</sup>: The longer the process goes on, the more the DM is rewarded with a more accurate valuation (assuming that a mis-valuation entails negative consequences). But following the bounded rationality paradigm, the process also causes opportunity costs and so the DM also needs to stop the process as soon as the temporary valuation is “good enough”. So for a valuation model, the DM needs to accumulate mental evidence for a lottery’s attractiveness and also decide when to stop sampling because the accuracy of the accumulated evidence is sufficient.

Rothschild (1974) developed a model where a “rational” DM searches for a minimum price from an unknown distribution with search costs. It features a DM using Bayesian updating of priors over possible price distributions that she infers from sampling prices. Thereby, the DM has a probability distribution of expected prices to observe next. Based on the expected value of the next price sample and the currently observed price, the DM can then assess if the expected price reduction of sampling again is worth the investment of the search cost. This might be able to be adapted to a search where a DM is not searching for the minimum price but for the median price of a distribution. Then, the DM would trade off inaccuracy in her estimation against the cost of more sampling. But this process is computationally intensive (Koulayev, 2013). Therefore, the assumption that a DM simultaneously holds priors over probability distributions is hard to reconcile with assumptions of a DM’s limited computational power.

Instead, two simpler approaches have been used in the bounded rationality literature so far. First a “Full Sampling” approach, where a DM accumulates samples of mental evidence and stops at some point to summarise the mental evidence into a final valuation. “Full Sampling” implies that a valuation is entirely built on mental samples. This approach is used by Query Theory to explain the WTA/WTP gap, described in section 2.5.2.1. Second, a “Choose and Adjust” approach, where a DM starts off with an initial sample of mental evidence for a valuation which is then gradually adjusted towards a final valuation through an iterative choice process.

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<sup>10</sup>For an overview of the exploration–exploitation dilemma in decision making, see Cohen et al., 2007)

“Choose and Adjust” implies that a valuation is built on a single mental sample that is then iteratively adjusted based on a choice process. This approach is used by the Sequential Value Matching Mechanism (Johnson and Busemeyer, 2005; section 2.5.2.2) and the Stochastic Pricing Model (Blavatskyy and Köhler, 2009a, 2009b; section 2.5.2.3).

### 2.5.2.1 Query Theory

Johnson et al. (2007) developed Query Theory to model a valuation process where a participant is either asked for a WTA or WTP for an object, e.g. a coffee mug. It is regularly observed in experiments that WTA values for objects are higher than WTP values (Kahneman et al., 1990). Johnson et al. assume that the valuation depends on a mental query process that 1) samples positive and negative aspects about the object’s attractiveness from memory, such as the usefulness of a coffee mug or the burden of holding on to it; and 2) samples similar evidence for receiving a specific money amount, such as being able to spend more or not getting the object’s entire worth in money. They then explain the WTA/WTP gap through the “query order” of that process and because the process stops before it is complete. They assume that a WTA task triggers a query order, which first samples value-enhancing evidence for the object’s attractiveness as well as value-decreasing evidence for money attractiveness. This supports higher WTA values. Only later in the process will mental evidence suggesting lower WTA values get sampled. But the process stops before sampling all possible mental evidence, so some evidence that supports lower WTA values is ignored. Therefore, WTA values are biased upwards. The reverse query order then applies to a WTP task, resulting in lower WTP values and the commonly observed WTA/WTP gap.

Testing this in a WTA/WTP experiment, Johnson et al. not only asked participants for WTA and WTP values but also to list the aspect that they considered in their valuation. They found that both WTA and WTP values crucially depended on the types of aspects listed first by participants, whether or not participants actually owned the object at the time. Furthermore, this dynamic also

occurred when participants were prompted to list either positive or negative aspects first, also whether or not participants actually owned the object at the time. Based on this evidence, Johnson et al. conclude that the query order of a mental valuation process fundamentally explains the WTA/WTP gap.

However, Query Theory has not yet been applied to the preference reversal phenomenon. If a DM's CE is between WTA and WTP for a lottery, this CE value can only be overvalued compared to the DM's SI point if choices rely on yet another query order. But Query Theory offers an example where WTA/WTP valuation behaviour can be explained entirely through a sampling process.

### **2.5.2.2 DFT and the Sequential Value Matching Mechanism**

Johnson and Busemeyer (2005) develop a model for a DFT decision process for an equivalence task, called the Sequential Value Matching mechanism (SVM mechanism). Note that DFT in its basic form (Busemeyer and Townsend, 1992; Busemeyer and Diederich, 2002) can already predict CEs through a partly similar matching process but this theoretical process was altered to allow the process to behave differently for WTA, WTP, and CE valuations (as well as for probability equivalents). This section will discuss the most advanced version, developed by Johnson and Busemeyer (2005).

Initially, a DM starts off with a potential "candidate value" as a potential amount for an equivalence task. This candidate value is sampled once from a distribution over the lottery's range and differs according to the task type. All distributions of potential candidate values take the shape of a bell curve. The distribution's mode for CE candidate values lies in the middle of the lottery's range. But the mode for WTA candidate values lies at the upper end of the lottery range while it lies at the lower range for WTP candidate values. So the DM tends to start the process with different candidate values, depending on the task type. After sampling the candidate value, the DM then introspectively decides whether she prefers the candidate value or the lottery in a straight choice.

This decision process is modelled through a DFT choice, but with three

possible outcomes: 1) prefer lottery, 2) prefer candidate value, 3) indifference. Unless indifference is an outcome of the comparison, the process carries on with further steps.

In each subsequent step, the DM adjusts the candidate value by an exogenous equal fraction of the lottery's range; either downwards in case it is preferred over the lottery or upwards in case of the other way around. Thereby, the candidate value is approaching the value of the underlying utility of the lottery step by step. DFT provides an exact theoretical value for this by assuming that the DM has a fixed underlying utility function. The sure amount that has the same utility as the lottery's underlying utility is this theoretical value, equivalent to an underlying CE.

Since DFT choices on average prefer the option with the higher underlying utility, choices will lead the adjustment process to weakly converge towards the underlying CE with an equal utility as the lottery. Due to the probabilistic nature of DFT decisions, the adjustment process can even reverse directions. But this happens with a lower probability. This weak convergence towards the underlying CE eventually stops as soon as a DFT choice concludes "indifference", i.e. that the candidate value and the lottery are indistinguishable in their utility with the required level of confidence. This models constraints in mental resources through a "cost of deliberation time", the comparison process is assumed to be stopped when a comparison has taken too long and hits an upper limit to the deliberation time for the comparison.

The initial distribution of candidate values as well as the pre-determined size of the steps in CE elicitation have a crucial impact on the DM's final valuation. Initial candidate values for CEs are distributed with the mode halfway between the maximum and minimum payoff. Due to the process stopping without ever fully reaching the underlying CE, this results in a bias towards the midpoint between the minimum and maximum payoff. Compared to an SI point of a risk-averse DM, this initial distribution of starting values is biased upwards in case of low winning probabilities.

Consider an SI point for the lottery and for simplicity, assume that a DM

is risk-neutral. Therefore, the DM will prefer any lottery over its expected value in 50% of choices and the lottery’s expected value is its SI point. However, in CE elicitations governed by the SVM mechanism, lotteries that win with a probability of *less* than 50% have an initial candidate that is on average *above* the lottery’s expected value, which in this case also is the SI point. So they tend to be overvalued compared to their SI point and expected value (and vice versa for lotteries with a winning probability of *more* than 50%). Even despite the risk-neutral preferences of the DM, this can lead to a preference reversal as the final valuation is biased to the starting value of the SVM process. Thereby, riskier lotteries are overvalued and safer lotteries are undervalued.

The step size is exogenously determined by dividing the range of the lottery’s maximum and minimum payoffs by a number that is the same for all types of lotteries. On the one hand, this leads to larger steps for lotteries with a higher variance, which might not allow the DM to “home in” with sufficient accuracy on their final valuation. On the other hand, if the step size is too low, the DM will need a much higher number of steps to arrive at their final valuation, with an increasing risk of stopping the process at a valuation much further from the SI point. Therefore, choosing the optimal step size involves a trade-off between predictions of speed and accuracy of the SVM process.

### 2.5.2.3 The Stochastic Pricing Model

Blavatskyy and Köhler (2009b) propose the stochastic pricing model (SP model): A model of lottery valuation that features adjustment steps as a proxy for deliberation time but does not have a stopping rule linked to speed-accuracy trade-offs. Since it was used to model participants’ behaviour under exogenous time pressure (Blavatskyy and Köhler, 2009a), this was not a problem. But it does pose a challenge in its comparison to the SVM mechanism and in the experimental context of endogenous deliberation times.

Blavatskyy and Köhler (2009b) assume that a DM draws evidence from a random utility distribution of CEs when valuing a lottery. Comparable to the SVM

mechanism, a starting value as the first Candidate CE is first drawn, but from a uniform distribution over the lottery’s range.

The DM then compares the lottery to the starting value, using a random utility function. If the lottery shows a higher utility, the preliminary CE is adjusted upwards, or vice versa downwards. Then, a subsequent comparison is made between the lottery and the updated candidate value. On average, the candidate value is updated towards the value of the median underlying CE.

The process stops as soon as the preference relation “switches direction”, i.e. as soon as the DM notices the candidate value becoming either “too high” (resp. too low) if it was “too low” (resp. too high) before. When the process stops, the DM settles on the mean of the last two candidate values as the final CE.

The variability in random preference relations causes this process to stop too early for the CE to reflect the DM’s median underlying CE. Instead, like in the SVM mechanism, final valuations are biased towards the starting value (when compared to the mean CE of the underlying distribution of utility functions).

As with the SVM mechanism, this effect is entirely controlled by the starting value. Assume a DM who chooses between a lottery and a sure amount much lower than the one at her SI point. This should lead the DM to start the valuation process at that precise sure amount and state a subsequent MSoP with a *negative* mismatch. I.e., unlike in Butler et al.’s (2014a) experiment, DMs logically imply with their MSoP a sure amount that is too low for them to be equally as attractive as the lottery on average. Vice versa, a “too high” sure amount would also introduce a negative mismatch in MSoP by biasing the implied CE upwards towards the starting value. If the downward adjustment process stops prematurely, the DM will still underreport their MSoP and therefore imply a CE that is too high compared to their SI point in choices.



## 2.6 Discussion

The current literature shows that the great majority of models fail to adequately describe both the preference reversal phenomenon and probabilistic choice behaviour. In addition, the MSoP mismatch observed by Butler et al. (2015) provides yet another behavioural regularity that cannot be explained by classic economic theory.

More sophisticated models of choice and valuation have been developed, which capture both probabilistic preferences and the preference reversal phenomenon. But these models, the SVM mechanism and the SP model, still leave some issues unaddressed. The SVM mechanism and the SP model do not use a full sampling process but instead a process where the DM samples once and then adjusts this sample through iterated choices. Because of this design, the prediction of the preference reversal phenomenon is entirely driven by anchoring effects from the initial sample.

If applied to MSoP values, they predict a negative MSoP mismatch, not the positive one observed by Butler et al. (2014a). Possibilities of effects from spill-over of mental evidence or consistency-seeking behaviour on MSoP mismatches have neither been explored in theory nor been tested in an experiment. The same is true for effects of individual reaction times on MSoP values. Current research also did not measure the *size* of MSoP mismatches and was only limited to detecting its occurrence.

Based on these gaps in the literature, the rest of the thesis works towards providing some answers to those issues. Chapter 3 simulates existing models and explores ways in which they could predict MSoP values. Chapter 4 develops a novel boundedly rational valuation model that uses a full sampling approach and makes novel predictions for MSoP mismatches, possible spill-over effects or consistency-seeking behaviour, as well as reaction time effects. Chapter 5 reports an experiment that quantifies discrepancies between SI points, CE values, and MSoP mismatches and thereby tests the different model predictions. Chapter 6 analyses whether longer deliberation times reduce lottery overvaluation and the frequency of preference reversals in the experiment. Chapter 7 provides a conclusion to the results.

## Chapter 3

# Existing Computational models applicable to Valuation

### 3.1 Introduction

Classic economic theory assumes that individual preferences towards risk that determine behaviour are largely consistent and stable across various contexts. But as described in the previous chapter, observations of preference reversals as well as probabilistic preferences challenge these assumptions. This chapter describes simulations of existing models that are applicable to model probabilistic choices, the preference reversal phenomenon, and MSoP mismatches. All models consider choices and valuations of monetary lotteries without negative payoffs.

The chapter considers choices between lotteries and sure amounts, lottery valuations, and direct choices between lotteries. Predictions about MSoP mismatches are based on lottery vs. sure amount choices. These simulations provide a set of predictions that are tested in an experiment in chapter 5 and can be compared to a novel valuation model described in chapter 4.

The chapter starts with the baseline case of EUT and the RP model, and shows how the RP model can also predict a positive MSoP mismatch. It then shows how the SVM mechanism and the SP model offer more advanced approaches. As both of these models have not yet been applied to an MSoP scenario, the chapter

shows how they can be extended to predict possible MSoP mismatches. With this approach, both the SVM mechanism and the SP model predict a negative MSoP mismatch following choices between lotteries and sure amounts.

### 3.1.1 Variables

The models described in this chapter can predict the following variables that are observable in an experiment.

**Probabilistic Choices and SI Points** Except for EUT, all models predict choice probabilities between lotteries instead of strict preference relationships. So they predict stochastic transitivity between choices in the Mosteller and Nogee setting (1951), which results in an SI point. In this chapter, choice simulations are limited to direct choices between the P-Bet and \$-Bet and choices between the lotteries and various sure amounts.

#### 3.1.1.1 Preference Reversals

While EUT and the RP model use the same approach to predict choices and valuations, satisfying procedure invariance, the SVM mechanism and the SP model assume different processes for choice and valuation. Therefore it is possible to compare lottery valuations to direct choices and SI points in order to detect the preference reversal phenomenon.

#### 3.1.1.2 The MSoP Mismatch

In this chapter, the RP model, SVM mechanism, and SP model are adapted to make MSoP valuations possible. All MSoP valuations are based on choice between a lottery and a sure amount. This works by simulating a choice first and then basing a valuation on that choice. E.g., in the RP model the same parameters for the utility function of a choice are used for a subsequent MSoP valuation. In case of the SVM mechanism and the SP model, the sure amount from the choice is used as the starting value for the subsequent valuation process. Possible MSoP mismatches

can then be identified by computing if MSoP values over- or undershoots the SI point.

### **3.1.1.3 Reaction Times**

The SVM mechanism and the SP model also feature a number of “steps” in the process, which can be interpreted as a proxy for reaction times in an experiment (see chapter 2, section 2.5.1). Reaction times provide another dimension for testable predictions but also a challenge for existing models. If sequential sampling models accurately predict individual choices and valuations but fail to predict corresponding effects on reaction times, this would be evidence that they fail to capture a fundamental aspect of a decision process. And even if the rest of the model predictions are accurate, they would still fall short of their goal of explaining a decision process.

Marginal effects offer a way to better explain decision making processes despite experimental noise (see section 2.5.1 for an explanation). The crucial assumption for this reasoning is that marginal increases in participants’ reaction times coincide with marginal increases in deliberation times. But if this assumption is true, reaction times offer an additional dimension to understand behaviour in experiments.

## **3.1.2 Implications for Valuation Models**

This chapter describes how models can deviate from classic economic theory to predict probabilistic preferences, preference reversals, and marginal effects of reaction times. In addition, adapting these models to predict MSoP mismatches offers the opportunity to provide novel explanations for yet untested phenomena such as spill-overs of mental evidence, consistency-seeking behaviour.

After a brief overview of traditional approaches in section 3.2, section 3 describes existing boundedly rational models that assume different decision processes for choices and valuations while still relying on the same set of underlying preferences. A comparison of the existing models is given in section 3.5. This provides the basis for developing a novel valuation model in the following chapter.

## 3.2 Expected Utility and the Random Preference Model

### 3.2.1 Expected Utility Theory

To put the model simulations in this chapter into context, the EUT model and the RP model are shown as baseline cases. The EUT model specifies a procedure-invariant preference between the P-Bet and the \$-Bet. We will use the utility function:

$$u(x) = x^{1-r} \quad \text{where } r = 0.185$$

on an 80% chance of winning £12 (expected value of £9.60) as a P-Bet and a 25% chance of winning £50 (expected value of £12.50) as the \$-Bet. This was because Loomes and Pogrebna (2016) report a strong preference reversal effect for these parameters.

The risk aversion parameter  $r = 0.185$  gives equal P-Bet and \$-Bet CEs of £9.13. Therefore, a choice between P-Bet and \$-Bet always depends on a tie breaker, in this case a 50/50 choice probability. And since these choices are consistent with lottery CEs, a preference reversal does not occur.

Also, both lotteries CEs are their SI points. The lotteries are always chosen over sure amounts below their SI point of £9.13 and always rejected for sure amounts above it. Choices between the lottery and the SI point's sure amount will also result in a 50/50 choice probability. So a graph of choice probabilities between the lottery and various sure amounts along the lines of Mosteller and Nogee (1951) will be a step function.

Any MSoP task following a choice can be answered by the CE difference between the options. E.g., if the P-Bet is chosen over £1, the MSoP will be £9.13 - £1=£8.13. This MSoP exactly reaches the SI point, resulting in a zero MSoP mismatch.

### 3.2.2 The Random Preference Model

This provides a comparison to the RP model (Loomes and Sugden, 1995), that can use the same model for the same pair of bets. This is possible by making the risk

aversion parameter  $r$  stochastic:

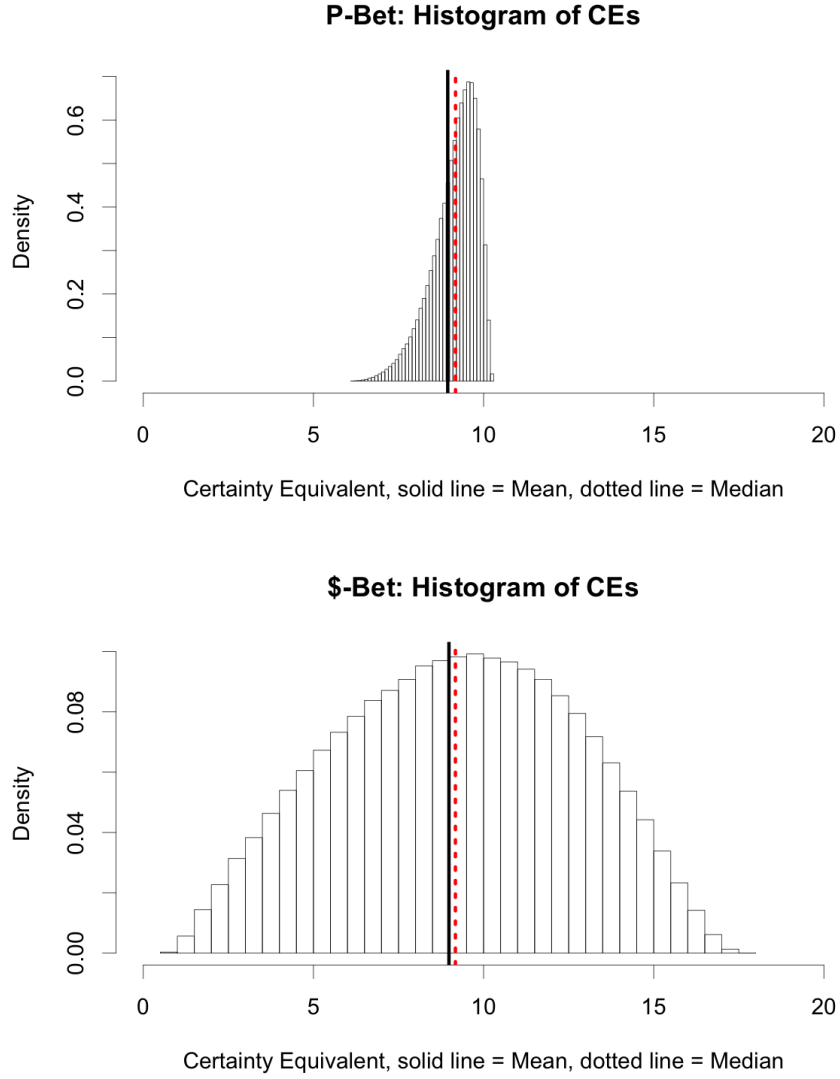
$$u(x) = x^{1-r} \quad \text{where } r \sim (\text{beta}(3, 3) \cdot \beta + (\alpha - \beta/2)) \quad \text{where } \alpha = 0.185 \beta = 1.0$$

Here, a *beta* distribution  $\text{beta}(3, 3)$  creates a random symmetrical bell-shaped  $r$  distribution;  $\beta$  is equal to its range and  $\alpha$  determines where the midpoint of the range lies.  $\text{beta}(3, 3)$  creates random values over  $[0; 1]$  with  $\text{mean} = \text{median} = 0.5$ . This distribution is then centred around  $\alpha$  with range  $\beta$ . This distribution is identical to the one used for the novel valuation model described in chapter 4, so it is useful to explain its properties in detail.

**Choices:** For each decision task, a DM then samples a single  $r$  parameter to use in the utility function. This  $r$  parameter then gives a CE, e.g. for a choice between the described P-Bet and \$-Bet or for a CE elicitation. As the median of  $r$  parameters equals  $\text{median}(r) = 0.185$ , half of the possible  $r$  values are either below or above it. Since a lower  $r$  parameter results in lower risk aversion, this means that the \$-Bet is preferred for  $r$  parameters below the median, i.e. in half of the cases. For the other half of cases with higher risk aversion, the \$-Bet is rejected instead. Therefore, the P-Bet is preferred over the \$-Bet with a probability of 50%.

Choices between bets and sure amounts can be inferred from the distribution of CE values from the lotteries, shown in figure 3.1. The P-Bet's CE range is  $[\pounds 5.95; \pounds 10.13]$  and the \$-Bet's CE range is  $[\pounds 0.64; \pounds 17.40]$ . This means that the P-Bet is always preferred over  $\pounds 5.95$  but never preferred over  $\pounds 10.13$  ( $\pounds 0.64$  and  $\pounds 17.40$  for the \$-Bet). Replicating Mosteller and Nogee's observation, the probability of choosing a lottery increases gradually from 0% at the lower bound of the range and until it reaches 100% at the upper end of the range. The median CE of both bets lies at  $\pounds 9.13$ . Therefore both bets will be preferred over  $\pounds 9.13$  exactly 50% of times, meaning that they have the same SI point at  $\pounds 9.13$ .

Figure 3.1: RP Model: Underlying CE sample distributions



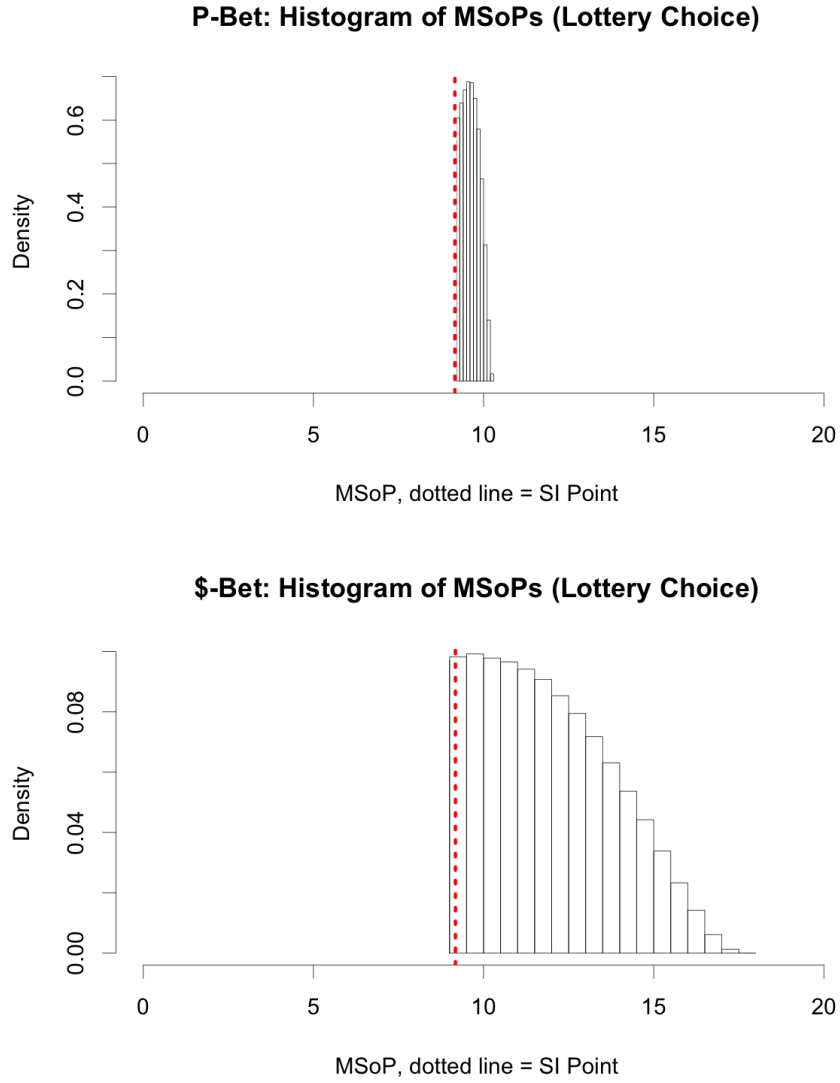
**Valuations:** Figure 3.1 shows the distribution of lottery CEs for the P-Bet and the \$-Bet (see section A.1 in the Appendix for an explanation why the CE distributions are not symmetric). These are the same CEs that govern choice behaviour. \$-Bet CEs are more spread out because of the higher variance of the \$-Bet. But picking a P-Bet and a \$-Bet CE at random leads to a probability of either CE being

higher at exactly 50% because the medians of both distributions are equal. So the RP model does not predict the preference reversal phenomenon. Although preference reversals occur, the asymmetric effect of a majority preference for the P-Bet in choices to a majority preference for the \$-Bet in valuations does not occur as observed by Grether and Plott (1979).

**Adjustments of Sure Amounts:** To illustrate MSoP values that are elicited after a choice, first consider a choice between a lottery and a sure amount at the SI point. By definition, the lottery is chosen 50% of times. Assume an occasion where the lottery is chosen based on a momentary  $r$  parameter. But based on that momentary preference, what will the MSoP be? If the DM uses the same  $r$  parameter for the MSoP elicitation, the MSoP value must equal the distance between the lottery's momentary CE and the sure amount because this is precisely what the difference in utility is worth in money to the DM. Also, the lottery CE *must* be higher than the SI point because the DM would have chosen the sure amount otherwise. So it is possible to infer from the lottery choice that the lottery CE must be within a subset of the CE distribution *above* the SI point. This is illustrated in figure 3.2 for both lotteries. P-Bet choices result in a mean MSoP mismatch of £0.39 (sd=0.23). \$-Bet choices result in a far larger mean MSoP mismatch of £2.82 (sd=1.84) because the \$-Bet CE distribution is larger.

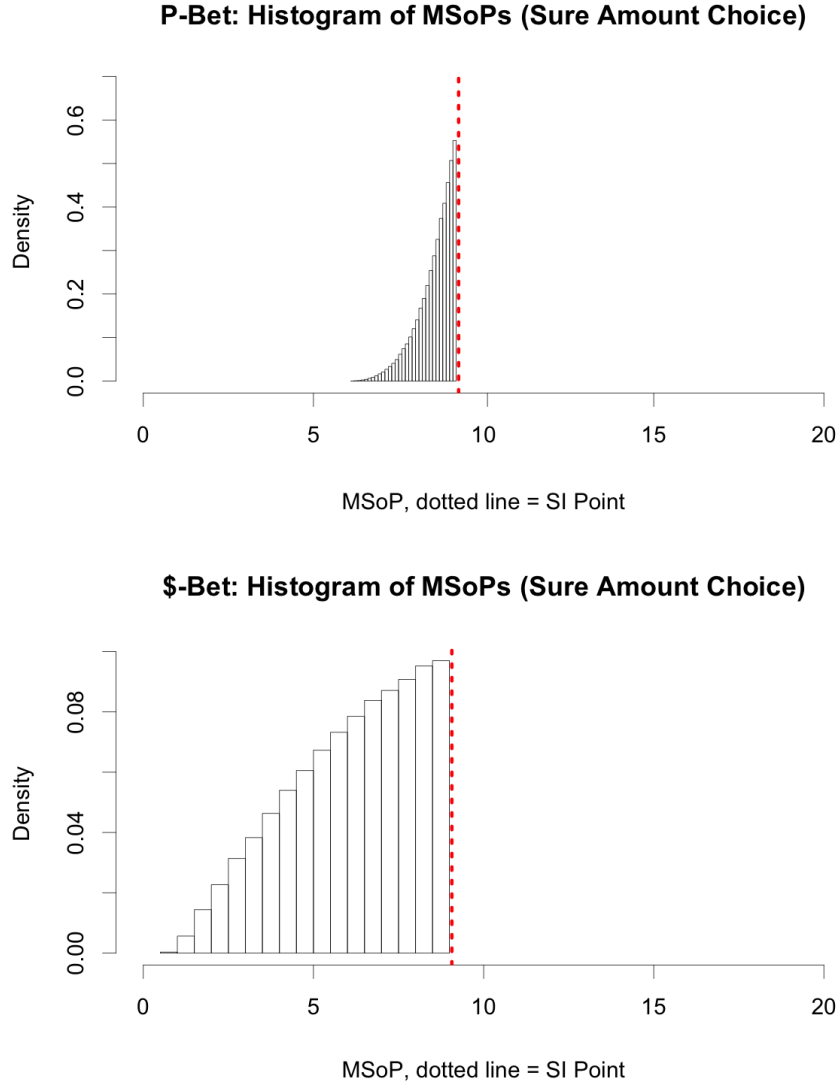


Figure 3.2: RP Model: MSoP Values after Lottery Choices



This effect goes into the opposite direction for sure amount choices. MSoP values for sure amount choices must be based on CEs below the SI point and are shown in figure 3.3. Here, P-Bet MSoPs overshoot the SI point with a mean of £0.64 (sd=0.54) after sure amount choices. Again, the mean mismatch is larger at £2.99 (sd=2.00) for the \$-Bet.

Figure 3.3: RP Model: MSoP Values after Lottery Choices



These predictions come from the assumption that a choice preference from the RP model spills over to an MSoP task. But if an MSoP value is elicited later after a choice between a lottery and sure amount, it is not sensible to assume that the DM would use the RP parameter from the past choice again for the MSoP. So if no spill-over effect is present, or if the DM starts the MSoP task with a “fresh” preference, DMs would report CEs from the distribution in figure 3.1. The result

after a choice between a lottery and a sure amount at the SI point would be a distribution of MSoP values that do *not* show an MSoP mismatch because half of the corresponding CEs would be either above or below the SI point.

So the RP model with assumption of spill-over effects can predict an MSoP mismatch and probabilistic choice and valuation behaviour. But the RP model does not predict the preference reversal phenomenon.

### 3.3 Johnson and Busemeyer (2005): The sequential Value Matching Mechanism (SVM Mechanism)

Johnson and Busemeyers' (2005) SVM mechanism, described in section 2.5.2.2 is simulated in this section and adapted to predict MSoP values.

The SVM mechanism was developed for straight valuation tasks, but it can also be adapted to model MSoP values (see section 3.3.4). Assume a choice task where a P-Bet is preferred over £1 for sure, followed by a prompt for the DM's MSoP. We assume that a DM that comes up with an MSoP according to the SVM mechanism will start off the valuation by comparing the bet to the first candidate value at hand, i.e. said sure amount £1. Thereby, it is possible to induce a valuation process with a specific starting value. The choice of starting value thereby generates MSoPs that logically imply valuations closer to it.

#### 3.3.1 SVM Mechanism Simulation

Following the parameters chosen by Johnson and Busemeyer (2005), the SVM mechanism has been simulated 1,000 times per task with a DFT process (Busemeyer and Townsend, 1993) based on the following utility function:

$$u(x) = \frac{x^{1-r}}{1-r} \quad \text{where } r = 0.2$$

The candidate value distribution is a truncated normal distribution with the mean at half of the lottery's range and a standard deviation of 10% of the

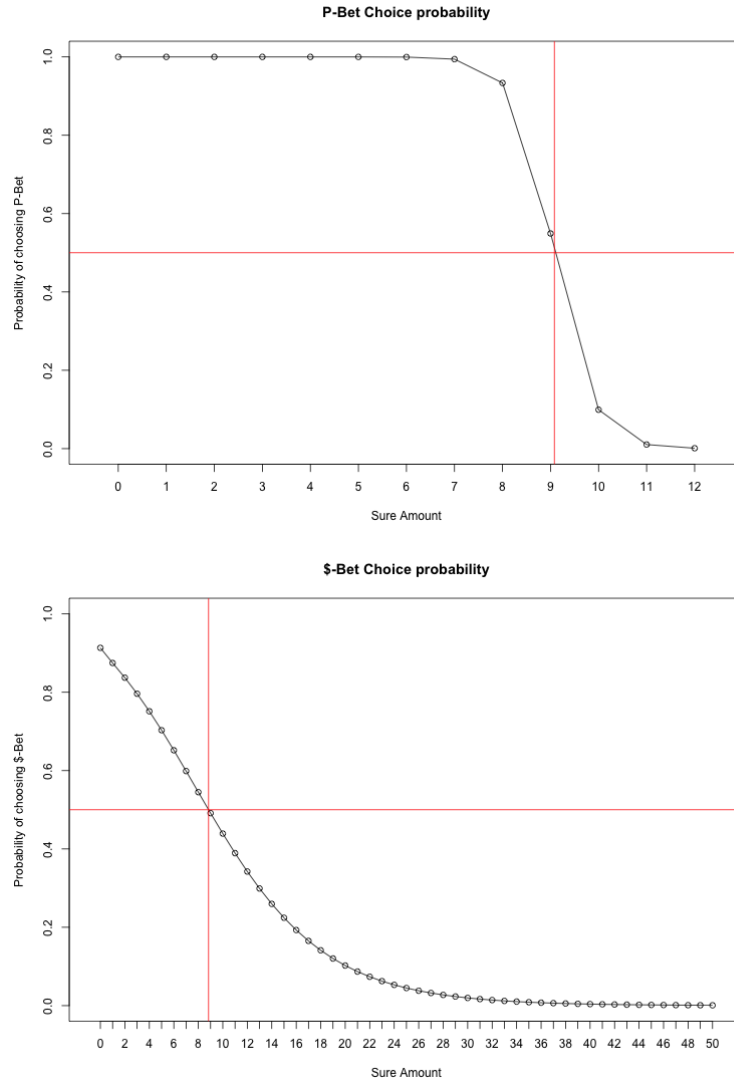
lottery range. Adjustment step size was 2% of the respective lottery range and the confidence threshold=25 for the DFT choice function. As in the original paper, the exit rate per sample unit (a proxy for seconds in deliberation time) was 2% and the step size per valence sample was 0.1.

Again, an 80% chance of winning £12 (expected value of £9.60) was used as a P-Bet and a 25% chance of winning £50 (expected value of £12.50) was used as the \$-Bet. The risk aversion parameter  $r = 0.2$  implies a SI point at £9.10 for the P-Bet and a SI point at £8.83 for the \$-Bet.

### 3.3.2 Choices

The DFT model (Busemeyer and Townsend, 1993) is the “core model” of the SVM mechanism and predicts binary choices. Figure 3.4 shows the choice probabilities of a DM choosing the respective lottery over various offered sure amounts. For the given parameters, DFT predicts a higher variability in choices for the \$-Bet. Note that although the \$-Bet has a lower SI point than the P-Bet, it is still more likely to be preferred over particularly high or low sure amounts than the P-Bet, which is a property of the RP model as well. Also, the \$-Bet is chosen over £0 in a minority of simulations. This is because for parameters including those of Johnson and Busemeyer’s (2005), DFT violates weak stochastic dominance for a minority of occasions (Busemeyer and Townsend, 1993). For a choice between P-Bet and \$-Bet, DFT predicts a 51% choice probability for the P-Bet.

Figure 3.4: DFT: Lottery Choice Probabilities



Note: Sure amounts for SI points are marked by the solid red line

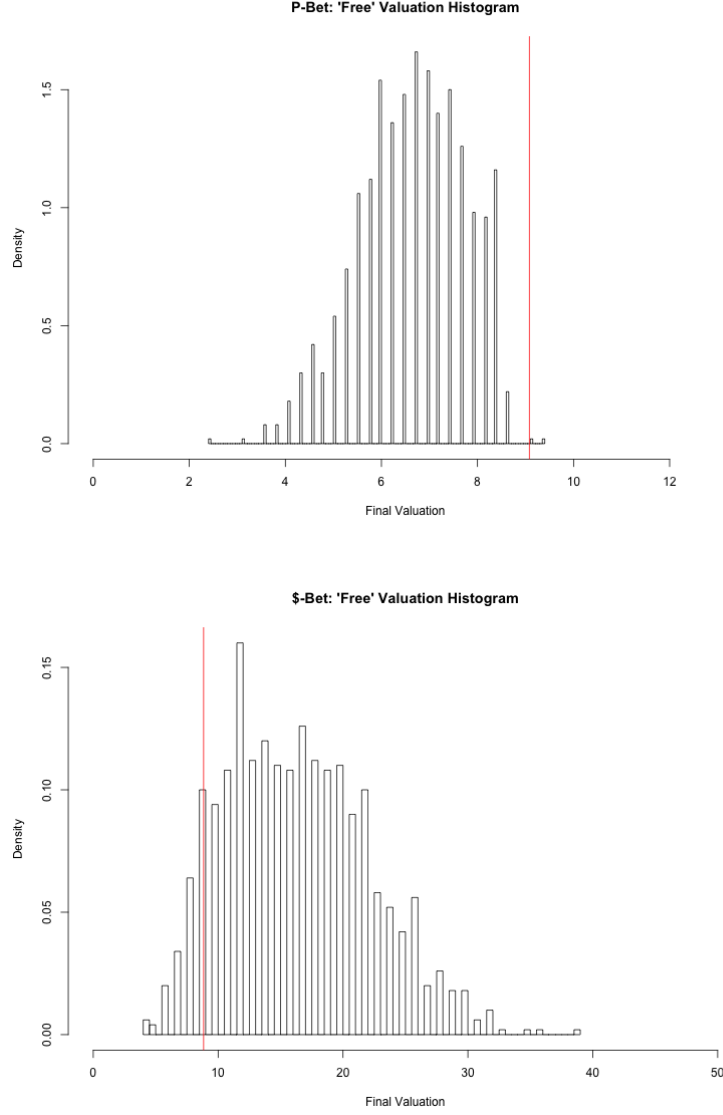
### 3.3.3 Direct Valuations

Valuations are assumed to be a standard CE elicitation as no candidate value for the sure amount is displayed to the participant. Since the SVM process models a convergence to a “true” CE value (in this case £9.10 for the P-Bet and £8.83 for the \$-Bet), candidate values will approach the sure amount at the SI with longer

deliberation time.

However, this approach will take longer for the \$-Bet, as most of its initial candidate values are around £25, halfway along the \$-Bet's payoff range. Since the average valuation process stops before reaching the SI point, the \$-Bet ends up being valued (mean=£18.67) higher than the P-Bet (mean=£6.67) in the vast majority (99%) of cases. The step size of adjustment (£1.0) also causes the \$-Bet valuations to be more clustered on specific values and the overall range of \$-Bet valuations is higher than that of the P-Bet.

Figure 3.5: SVM Mechanism: Lottery Valuations



Note: Sure amounts for SI points are marked by the solid red line

### 3.3.4 Adjustments of Sure Amounts: MSoP Values

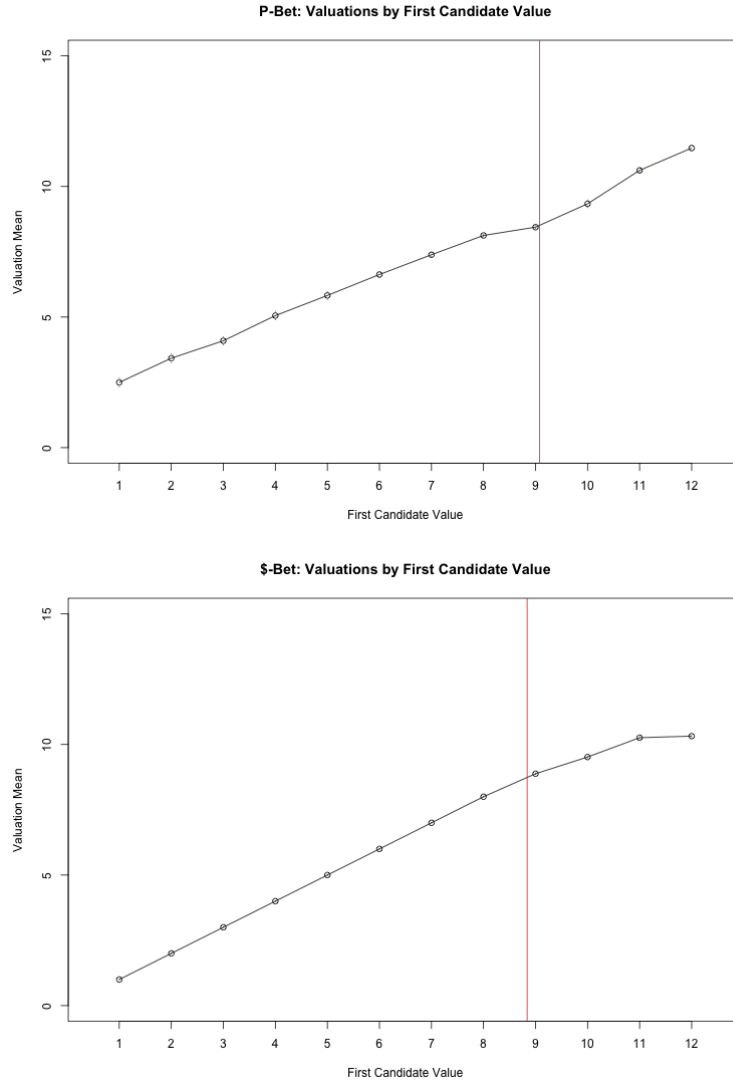
The SVM mechanism was not developed to be applied to anchored valuations but the nature of its adjustment process makes it possible to extend the SVM mechanism to also predict MSoP values. To achieve this, we assume that a valuation which im-

mediately follows a choice task is equal to a SVM mechanism with a pre-determined starting value. By considering a possible sure amount, the DM uses this value as a first candidate value and subsequently adjusts it to reach her final valuation. The SVM mechanism predicts that the resulting CE is closer to the first displayed sure amount than a direct valuation, similar to an anchoring effect. By inducing these candidate values through previous choices, on average the final CE of the lottery can be determined to be either lower or higher than the SI point (assuming that both lottery CEs are implied by the MSoP). Note that from this, a negative MSoP mismatch follows for *all* sure amounts: As final valuations are biased towards the starting value, they consistently fall between the starting value and the SI point. Therefore, resulting MSoP values consistently are too small to reach the SI point.

This prediction of a negative MSoP mismatch is also tested in chapter 5 (section 5.4.2). Note that in the case of choices between lotteries, Butler et al. (2014a) found a positive MSoP mismatch.



Figure 3.6: SVM Mechanism: Lottery Valuations dependent on initially displayed Sure Amount



Note: Sure amounts for SI points are marked by the solid red line

### 3.3.5 Deliberation Time and Valuations

Each generated valuation comes with a number of steps that were used in the decision making process. The minimum number of steps occur when a starting value is sampled and the process ends after the first comparison. In that case, the simulation

counts one adjustment step. With each subsequent adjustment the total number of steps increases by one.

We can observe a marginal effect of deliberation times on final valuations. Figures 3.7 and 3.8 give an overview of the results through violin plots of P-Bet and \$-Bet valuation distributions, minus the SI point and separated by sample sizes. As described by Johnson and Busemeyer’s (2005) for P-Bet and \$-Bet lottery types in general, larger steps numbers correspond with smaller distances to the SI point. But for all step numbers, median P-Bet valuations are below the SI point, while median \$-Bet valuations are above the SI point.

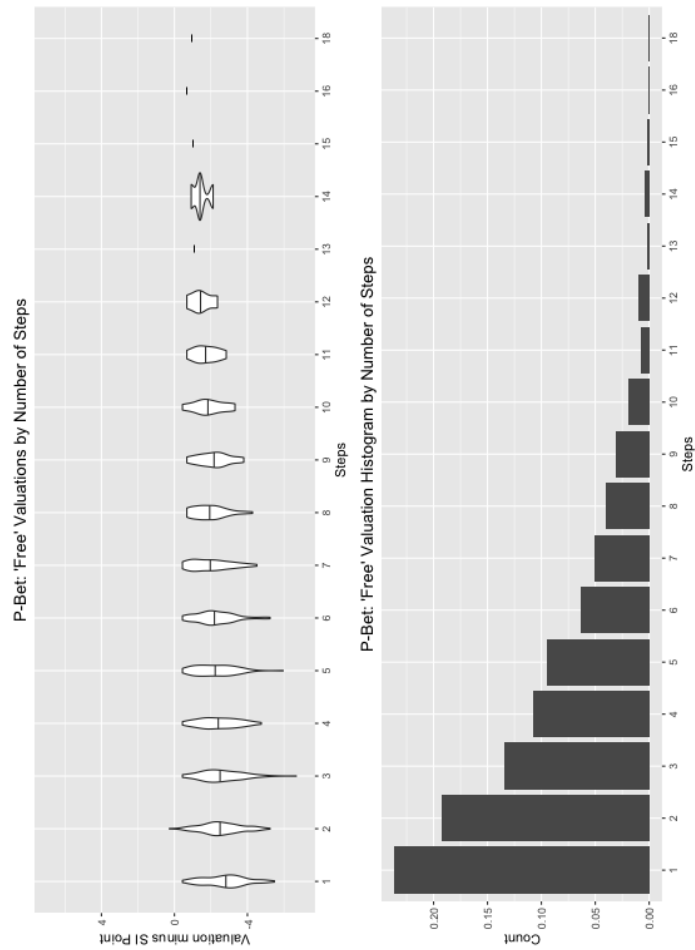
Median P-bet valuations minus the corresponding SI point increase with step number (£-2.84 to £-0.91 from 1 step to 18 steps) as final valuations move upwards to the SI point. The effect for the \$-Bet is stronger and in the opposite direction: Median \$-bet valuations minus the corresponding SI point decrease with step number (£14.16 to £-0.16 from 1 step to 50 steps) as valuations move downwards towards the SI point. P-bet valuations also have a median number of 3 steps, which is far lower than for the \$-Bet at a median number of 11 steps.

But in an experiment it is not known what underlying parameters determine a participant’s valuation behaviour. Interpreting the marginal effects of different step numbers on valuations gives predictions for experiments that are easier to test. Treating step number as a proxy for deliberation time, the SVM mechanism predicts that participants with individually-longer deliberation times will report lottery valuations that are closer to their SI point.

Figures 3.7 and 3.8 illustrate this. For both lotteries, higher sample sizes result in a convergence of the valuation towards the SI point. Larger sample sizes reduce the distance between a valuation and the SI point.

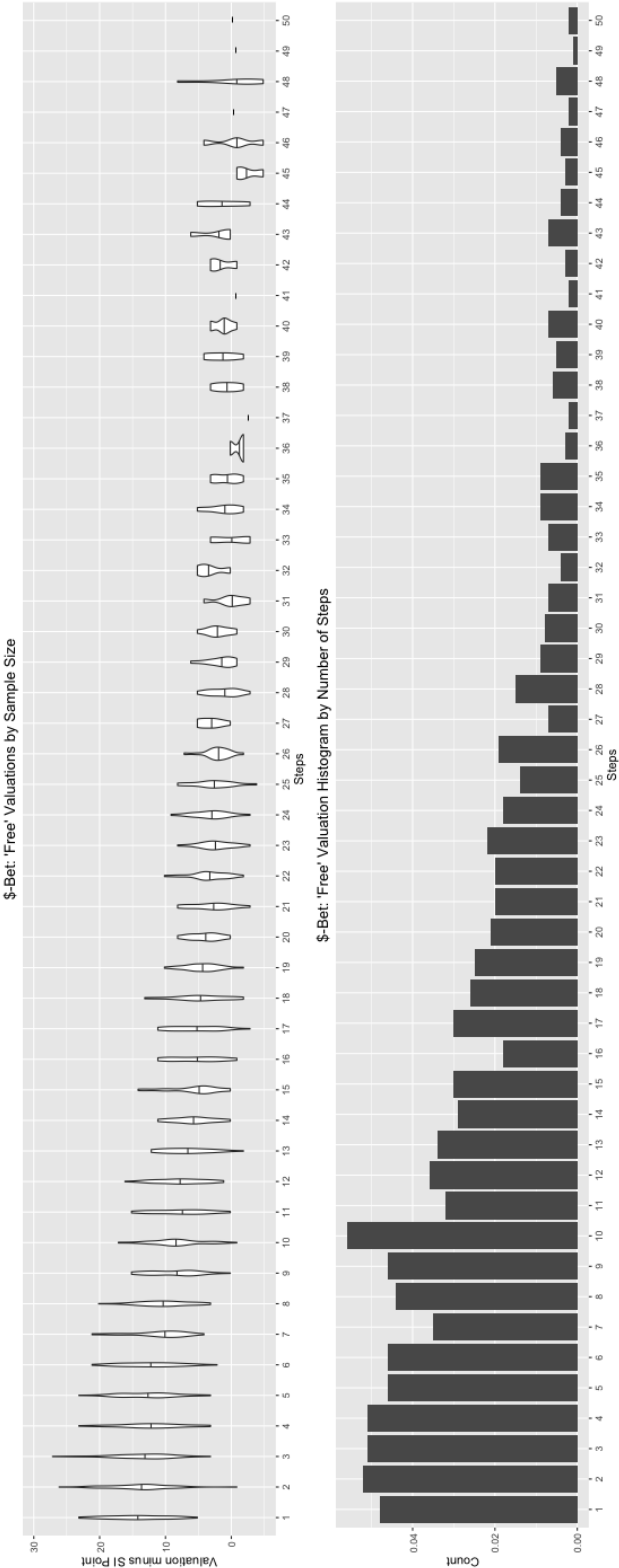
This effect is positive and weaker for the P-Bet but negative and stronger for the \$-Bet. In addition, \$-Bet valuations take longer than P-Bet valuations for the same participant. These marginal effects are qualitative predictions that can be measured by a yes/no approach at the individual level in an experiment (see section 4.5.4).

Figure 3.7: SVM Mechanism P-Bet: Lottery Valuations, their corresponding Number of Steps, and their frequencies



Note: Median values are marked by horizontal lines

Figure 3.8: SVM Mechanism \$-Bet: Lottery Valuations, their corresponding Number of Steps, and their frequencies



Note: Median values are marked by horizontal lines

### 3.4 Blavatskyy and Köhler (2009a, 2009b): The model of stochastic Pricing (SP model)

This section describes simulation results for the SP model, explained in section 2.5.2.3. It predicts a valuation process that resembles the SVM mechanism and is also biased towards a starting value but uses an RP model as a choice function instead. As with the SVM mechanism, the SP model can be adapted to predict MSoP values (see section 3.4.4).

#### 3.4.1 SP Simulation

Since Blavatskyy and Köhler’s model of stochastic pricing does not cover straight choices, a few additional assumptions need to be made for a simulation. Straight choice tasks always deal with a binary choice. This is identical to a single step in the SP model. Therefore, MSoP tasks are modelled as a valuation task with a specific starting value.

Following Blavatskyy and Köhler (2009b), we simulated each task 10,000 times to predict choices and valuations of DMs using the following random utility function:

$$u(x) = \frac{x^{1-r}}{1-r} \quad \text{where } r \sim \text{norm}(\mu = 0.2; sd = 0.4)$$

Again, an 80% chance of winning £12 (expected value of £9.60) was used as a P-Bet and a 25% chance of winning £50 (expected value of £12.50) was used as the \$-Bet. The parameters imply a SI point at £9.10 for the P-Bet and a SI point at £8.83 for the \$-Bet. Consequently, the P-Bet is preferred to the \$-Bet in choices (in 60% of cases). Both are above the respective lottery’s expected value because, as in Blavatskyy and Köhler (2009b), the individual mean risk aversion parameter is risk-seeking<sup>1</sup>. As in the original simulation, the step size was set to 10% of the respective lottery’s range.

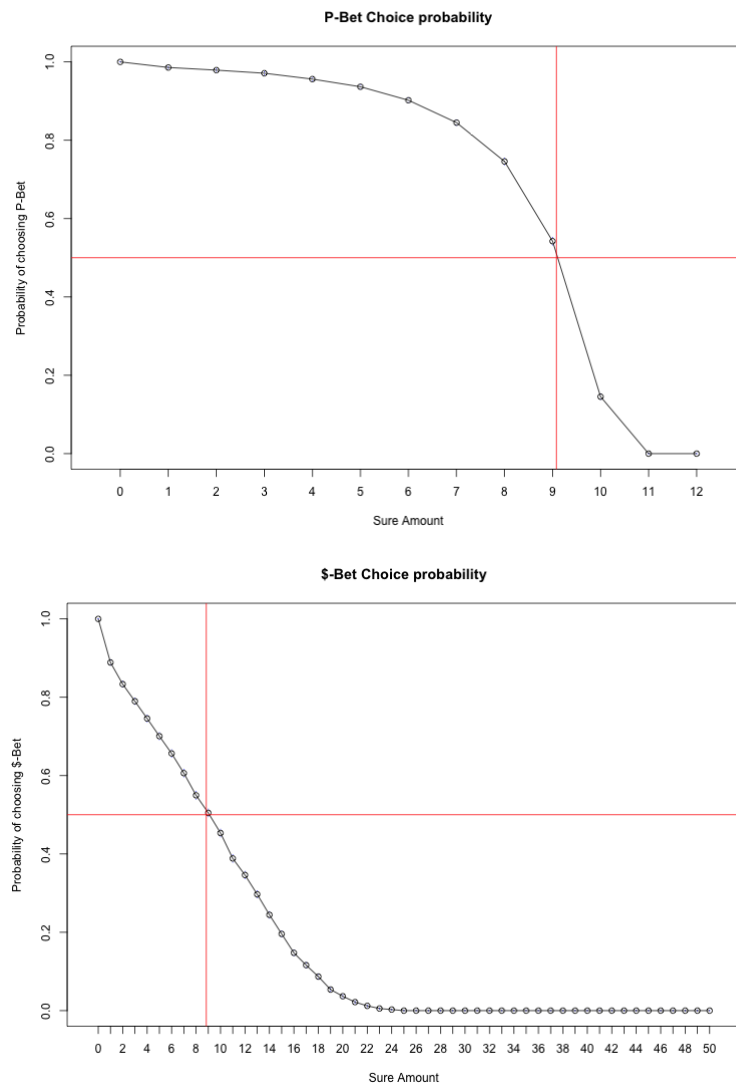
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<sup>1</sup>On the condition that any  $r \geq 1$  are eliminated.

### 3.4.2 Choices

A straight choice task would be a special case of the SP model, a single random utility preference relationship between the lottery and the sure amount. Both lotteries show a gradually decreasing probability of choosing the lottery over increasing sure amounts:

Figure 3.9: SP Model: Lottery Choice Probabilities



### 3.4.3 Direct Valuations

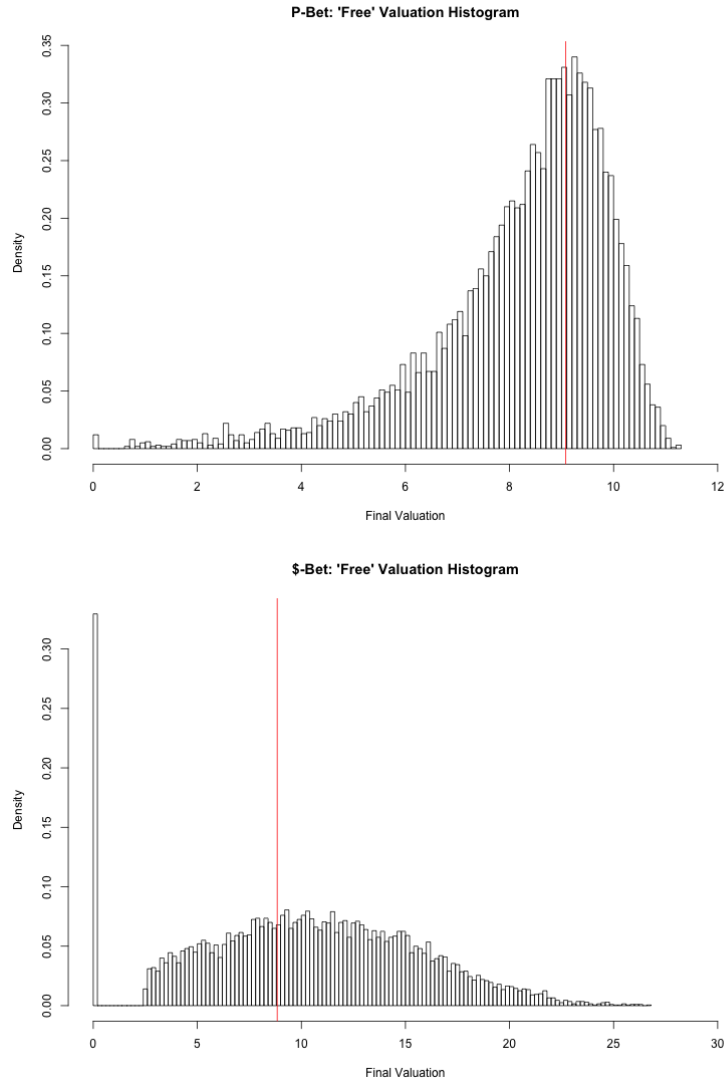
The CE predictions of the SP model do predict a preference reversal from preferring the P-Bet over the \$-Bet in 60% of choices to valuing the \$-Bet over the P-Bet in 53% of valuations. The mean P-Bet valuation was at £9.92 (med=£10.41). The mean \$-Bet valuation was at £10.5 (med=£10.63) with a much larger variance ( $var(\$) = 18 > 4 = var(P)$ ). Higher mean \$-Bet valuations were predicted despite some \$-Bet valuations at £0, in instances where a \$-Bet was rejected by the SP model in favour of £2.50 or less<sup>2</sup>. Note that the mean P-Bet valuation is below the P-Bet's SI point while the mean \$-Bet valuation is above:

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<sup>2</sup>If a \$-Bet candidate value at or below £2.50 is rejected, the next candidate value will be at or below £0. The SP model will always reject this next candidate value below £0 with certainty. Then, the final valuation, equal to the mean of the last two candidate values, would be equal to a number at or below £0. As the SP model rules out final valuations below £0, valuations in these cases are set to £0.

A similar scenario occurs if a P-Bet candidate value is rejected in favour of £1.20 or less.

Figure 3.10: SP Model: Histogram of Lottery Valuations



Note: y-axes have different lengths

Note: Sure amounts for SI points are marked by the solid red line

#### 3.4.4 Adjustments of Sure Amounts: MSoP Values

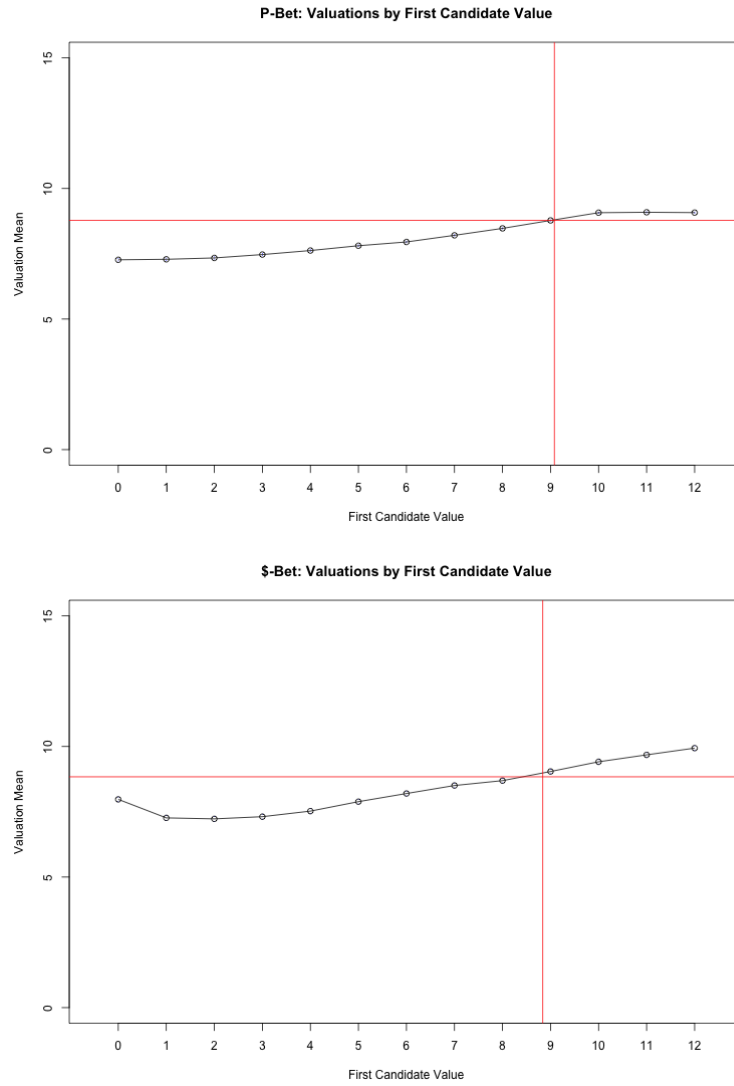
Similar to the SVM mechanism, the SP model was not built to predict MSoP values but can be altered to do so. Choices over a sure amount provide the initial candidate value and also determine the direction of adjustment. E.g. choosing a sure amount



of £10 over the \$-Bet will result in a final valuation below £10. Choosing initial candidate values entirely controls the preference reversal. Even without incorporating the direction of adjustment, the final valuation is biased towards the initial sure amount (and more so for the \$-Bet). Again, this implies a negative MSoP mismatch. This effect direction is the same as for the SVM mechanism (section 3.3.4).

Mean valuations are only higher for the \$-Bet when its starting values are above the \$-Bet SI point and also P-Bet starting values are below the P-Bet SI point. Otherwise, the preference reversal disappears (see figure 3.11).

Figure 3.11: SP Model: Lottery Valuations dependent on initially displayed Sure Amount



Note: Sure amounts for SI points are marked by the solid red line  
Note: Below: Mean Steps

### 3.4.5 Deliberation Time and Valuations

Similar to the SVM mechanism, the SP model predicts a number of steps along with valuations, which can also be understood as a proxy for deliberation time. But since the models stops at the first change in adjustment direction, the earliest a process

can stop is at two steps. Step numbers range from two to ten steps for both lotteries (see figures 3.12 and 3.13: the median number of steps is 4, both for the P-Bet and the \$-Bet). The P-Bet requires a mean step number (mean=4.13) that is only 14% smaller than for the \$-Bet (mean=4.77)<sup>3</sup>.

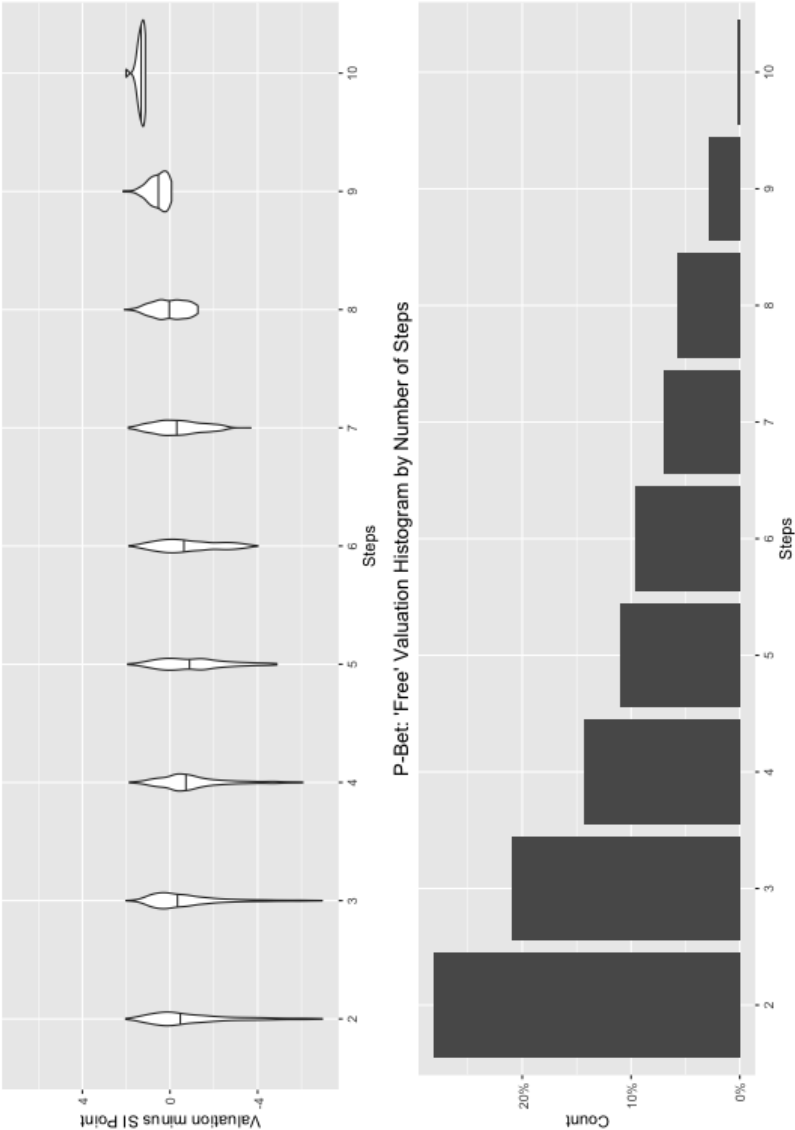
Note that Blavatsky and Köhler (2009b) do not use this model to predict endogenous deliberation times. Instead, they used the model to predict the path of convergence of candidate values towards a final valuation. And they do find that valuations converge onto a final value at least weakly in the majority of cases. Therefore in a strict sense, the SP model does predict step numbers, but these step numbers cannot be used as a proxy for deliberation times.

Still, this simulation can give a prediction for the *marginal* effects of longer or shorter deliberation times. From minimum to maximum step number, P-Bet valuations increase from £0.46 below the SI point to £1.30 above it while \$-Bet valuations show a decrease from £1.29 above the SI point to £0.56 below it. Applied to deliberation times, this predicts the marginal effect that P-Bet valuations are higher for individually-longer deliberation times whereas \$-Bet valuations are lower.

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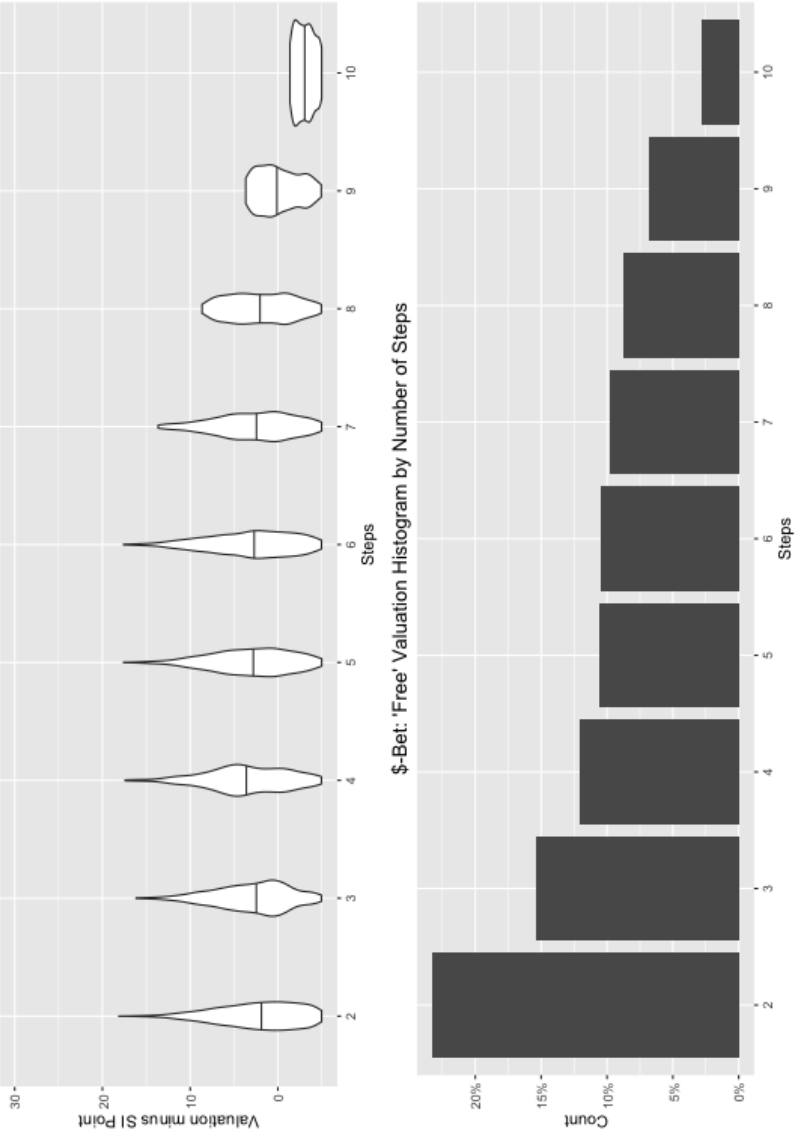
<sup>3</sup>Despite this small difference, a Kolmogorov-Smirnov test reports a highly significant difference in distributions ( $D=0.1231$ ,  $p<0.0001$ ). This could offer a testable prediction for an experiment. But even without any additional experimental noise, a t-test would require 160 participants to report a difference in means with an expected p-value of 5%. Therefore, the predicted difference in step number cannot be extended to a prediction of a difference in deliberation times between the P-Bet and the \$-Bet with these parameters. However, other parameters are possible that would give predictions where it is easier to distinguish between P-Bet and \$-Bet deliberation times. But to simplify the comparison to the model description in the original paper, parameters are left unchanged.

Figure 3.12: SP Model P-Bet: Lottery Valuations, their corresponding Number of Steps, and their frequencies



Note: Median values are marked by horizontal lines

Figure 3.13: SP Model \$-Bet: Lottery Valuations, their corresponding Number of Steps, and their frequencies



Note: Median values are marked by horizontal lines

## 3.5 Comparison of existing Models

### 3.5.1 Differences

The SVM mechanism and the SP model use different probabilistic decision models as a “model core” to predict comparisons between a lottery and its candidate values that are necessary for the adjustments in the valuation process. The SP model uses a RP model that provides a strict preference for each comparison. The SVM mechanism uses DFT instead, which can also provide an indifference response for a comparison.

This difference also leads to different stopping rules. While the SVM mechanism stops as soon as a comparison yields an indifference response, the SP model stops the process as soon as a comparison switches the direction of the necessary adjustment.

The distribution of potential starting values is also slightly different across the models, although its average effect is the same. The SP model assumes a uniform distribution of starting values along the lottery’s range. The SVM mechanism assumes a unimodal distribution over the same range with its centre halfway between the lottery’s minimum and maximum payoff. But in both cases, this leads the average starting value to lie below the SI point for low-variance lotteries and above it for high-variance lotteries.

### 3.5.2 Similarities

The two existing models both use a probabilistic decision model as a “core” to predict the comparisons that are necessary for the adjustments in the valuation process. In both cases, this “core” represents a “true” underlying preference that would also be elicited through the valuation process if it would go on indefinitely. Comparisons between valuations based on infinite sampling would then reflect the average preference in choices.

Both models use a single mental sample of a starting value that is crucial in the process because final valuations are biased towards it. This starting value is

compared to the lottery via the core model and on average adjusted towards the SI point. But on average, the process stops before the candidate value reaches the SI point. Therefore, average valuations are predicted to lie between the starting value and the SI point. Since both models assume that starting values are below the SI point for the P-Bet and above the SI point for the \$-Bet, this predicts the preference reversal. The SI point is higher for the P-Bet than for the \$-Bet, causing an average preference in choices for the P-Bet. But valuations show the opposite pattern with average valuations that are higher for the \$-Bet than for the P-Bet. Therefore, the \$-Bet is overvalued relative to the SI point while the P-Bet is undervalued. Note that this is consistent with Tversky et al.'s (1990) conclusion about over- and undervaluing in the preference reversal phenomenon. However, Loomes and Pogrebna (2016) instead found an overvaluation of both P-Bets and \$-Bets when inferring SI point estimates.

Since valuations are biased towards the starting value, both models predict a negative MSoP mismatch after lottery vs. sure amount choices in our specification. Therefore, these models predict that Butler et al.'s (2014a) positive MSoP mismatch will not be replicated following choices between lotteries and sure amounts. This applies both to positive adjustments of sure amounts (MSoP tasks after lottery choices over sure amounts) and negative adjustments (resp. sure amount choices).

Furthermore, as the models are limited to a single sample combined with iterative choice as information gathering mechanism, they do not incorporate overconfidence or cognitive dissonance effects, which would have an impact on the accumulation of mental evidence.

## Chapter 4

# A novel Model of Boundedly Rational Valuation

### 4.1 Introduction

This chapter develops a novel model of sequential sampling to predict valuation behaviour in experiments featuring monetary lotteries. It can be linked to an existing choice model and makes novel predictions for how choices can influence subsequent valuations. In addition, the model also incorporates spill-over (section 4.4.1) and consistency-seeking effects (section 4.4.2) as additional factors that might be able to predict yet untested phenomena in experiments.

Unlike the SP model and the SVM mechanism described in the previous chapter, the valuation process is entirely based on sequentially sampling mental evidence from the same distribution that is used for choice predictions. The starting value for a valuation process is also not exogenously generated but drawn from the distribution of mental evidence. In addition, the process stops endogenously based on the accumulated mental evidence.

Connected to BREUT as the choice model, the valuation model uses the same distribution of mental evidence in the form of CE values. It predicts weak effects in line with the preference reversal phenomenon, despite obeying weak stochastic transitivity in choices. This is the result of an overvaluation of both P-Bet and



\$-Bet with a stronger effect for the \$-Bet. This is a difference to the models described in chapter 3, which is in line with the result from Loomes and Pogrebnia (2016). The model predicts a positive MSoP mismatch after choices, which is independently possible in two ways: via spill-over effects from choices onto valuations and consistency-seeking effects resulting from choice displays.

At this stage, the model is built to predict decision behaviour over sure amounts and lotteries with positive payoffs and known winning probabilities. All choices are between two alternatives and MSoP values consider a single lottery against a sure amount. This model is simulated in section 4.3.3 and compared to existing models in section 4.5. Section 4.6 discusses its predictions and contribution.

## 4.2 Navarro-Martinez et al. (2017): Boundedly Rational Expected Utility Theory (BREUT)

Navarro-Martinez et al. (2017) follow a sequential sampling approach for Boundedly Rational Expected Utility Theory (BREUT), a choice model where a DM accumulates evidence on her preference through samples of CE differences between options. These CE differences are generated by an RP model, resulting in probabilistic preferences in choices.

BREUT has three components:

1) An RP model as source of mental evidence: This is provided by sampling from a distribution of CRRA EUT functions<sup>1</sup> with randomness in their parameters, so no “true” underlying preference as in DFT exists. Each sample returns a von Neumann-Morgenstern function with a random risk aversion parameter. This function then provides a CE for each option at hand. The DM samples her preference relation in the form of the CE difference  $\Delta CE_k$  between options according to the sampled function at step  $k$ .

2) With the CE difference as a benchmark for the strength of preference, an additional sample is generated at each step and each time  $\Delta CE_k$  is averaged. This

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<sup>1</sup>CRRA := constant relative risk aversion

“accumulates evidence” in favour of one option or the other at each step  $k$ .

3) With each sampling step, a stopping rule is checked to determine if the amount of evidence in the form of CE differences is sufficient to justify a choice for the option with the higher mean CE. A DM stops sampling and chooses the option as soon as a preference for an option is distinguishable enough from zero, given a sufficient level of confidence. This is done at each step  $k$  by a t-test: if the test-statistic is above a confidence threshold, the process stops at the final step  $K$ . To check for this, the t-test statistic of the sample means is computed:

$$T_K = \frac{\text{mean}(\Delta CE_1, \dots, \Delta CE_K)}{\text{std.dev.}(\Delta CE_1, \dots, \Delta CE_K) / \sqrt{K}}$$

And the absolute of the test statistic is compared to the cumulative distribution function of the  $t$ -distribution (with  $(k - 1)$  degrees of freedom). If the test statistic exceeds the confidence level, the process stops at  $K$ :

$$F_{K-1}[|T_K|] \geq \text{confidence}_K \iff \text{stop at } K$$

To model constraints in time and attention, the necessary confidence level  $\text{confidence}_k$  decreases by an individual-specific rate  $d \in [0; 1]$  throughout the process. So the longer the sampling process takes, the more willing the DM is to take a choice that might not reflect a preference that would be elicited on average.

Confidence is reduced each time the stopping rule is checked, so the earliest opportunity to check the stopping rule is at sample size 2. This results in  $\text{confidence}_k = 1 - d(k - 1)$  with confidence decreasing in sample size  $k$ .

## 4.3 Extending BREUT to Valuation

### 4.3.1 Direct Valuations

BREUT already involves a component that gathers evidence usable for a valuation. Since the model accumulates CE differences, these are easily convertible to be used for the valuation of a single option.

So a BREUT valuation process could leave the generation of possible CEs unchanged (component 1) and instead use a different evidence accumulation mechanism (component 2) as well as an adapted stopping rule (component 3).

### **Component 1: Underlying Distribution of Preferences**

CE values for a single lottery with positive payoffs can be generated by the same RP Model that is used for BREUT choices. Similarly to BREUT, this would generate an underlying distribution of CE values for a lottery. While the distribution of EUT functions does not change, any lottery will result in its own unique distribution of corresponding CE values (depending on its payoffs and their probabilities).

That way, the valuation process relies on the same distribution of underlying CEs as the choice process for each specific lottery. And the DM also samples CE from this distribution for a lottery valuation. Therefore, a stable *underlying* CE distribution is an unchanging characteristic of the DM.

### **Component 2: Accumulation of Evidence**

With each sampling step  $k$ , the sample would include an additional CE value as mental evidence for the lottery's valuation to the DM. A straightforward way to accumulate this mental evidence is the updated mean of the CE sample. So at each sampling step, the mean of the sample is equal to the DM's candidate value for a final valuation.

### **Component 3: Stopping Rule**

Finding an appropriate stopping rule is more difficult. The DM is assumed to "satisfice" and stop the process as soon as the valuation is precise enough, given her required level of confidence. But unlike in BREUT choices, there is no other option with which CE values can be compared to judge their precision. So the stopping rule can only rely on the CE sample of a single lottery.

The coefficient of variation is a frequently used simple indicator for the accuracy of a sample that performs well in describing behaviour (Weber et al., 2004)

and can simply be estimated by using:  $std.dev.(sample)/mean(sample)$ . The coefficient of variation also becomes smaller the larger a sample becomes. Throughout the sampling process a sample's variance decreases and thereby reduces the coefficient of variation, serving as a proxy for a reduction in imprecision.

This characteristic of the coefficient of variation can be used for a stopping rule. We assume that at each step  $k$ , the DM estimates the coefficient of variation from the CE sample as a measure of the likelihood that an additional CE value will change the sample mean, i.e. measuring imprecision. If the estimated coefficient of variation is high, it is more likely that the estimation is inaccurate and requires more sampling. If not, an additional CE value is less likely to change the sample mean because the estimation might already be precise. It follows that a higher coefficient of variation is an indicator that an estimation is imprecise.

Therefore, higher coefficients of variation correspond to lower levels of confidence in the estimation. If at step  $k$  the coefficient of variation is too high for the DM to be confident enough in the estimation's accuracy, the DM will carry on sampling with the next step  $(k + 1)$ . But if the DM accepts this level of imprecision, she will stop the process at this step.

So as soon as this coefficient of variation is small enough, the DM will stop the valuation process subject to the level of imprecision she will accept at the final step  $K$ . This can be expressed by the following rule:

$$\frac{std.dev.(CE_1, ..., CE_K)}{mean(CE_1, ..., CE_K)} \leq accepted\ imprecision_K \iff stop\ at\ K$$

Similar to BREUT in choice, we assume that the required level of confidence will decrease over time, thereby increasing the level of accepted imprecision. In order to compute a candidate CE mean as a candidate value, the DM will sample two CE values and decrease the confidence level  $confidence_k$ . For parsimony, this happens at the same individual-specific rate  $d$  as in choices. A level of total confidence corresponds to an allowed coefficient of variation of zero, i.e. the process can only stop if the standard deviation is zero. But with each additional step in the process,

the accepted level of imprecision increases from this lower bound. Note that the model does not have a finite parameter value for  $d$  that leads the DM to always accept the first sample mean as final valuation (such a specification is possible for BREUT choices). In addition, the lower bound can theoretically be any number, even below zero. But to keep the model simple, a lower bound of zero is used as an implicit parameter.

The  $d$  parameter results in  $accepted\_imprecision_k = d(k - 1)$  increasing in sample size  $k$  and thereby accepting a higher coefficient of variation to stop the process as sampling goes on. This serves as a measure of impatience. This also corresponds to impatience in choices where the same value for  $d$  is used. As the process goes on for longer, the DM will accept more and more noisy samples as sufficiently accurate for the valuation (other things being equal, as increasingly large samples also become less noisy throughout the process). For higher values of  $d$ , DMs become more impatient more quickly throughout the process.

Adding these two extensions to BREUT is sufficient to generate valuations. The remainder of this section will describe how lottery characteristics and model parameters influence predictions. Section 4.4 will show how the valuation model can allow for choices and valuations to be combined to model MSoP elicitations.

### **The Impact of Variance on the Stopping Rule**

Note that the coefficient of variation shows a higher sensitivity to positive deviations than to negative deviations. E.g. the set  $\{8, 10\}$  has a higher coefficient of variation than the set  $\{10, 12\}$  even though both samples have the same variance<sup>2</sup>. Still, it is a relative measure, so this mismatch is the same for the scaled up sets  $\{80, 100\}$  and  $\{100, 120\}$ . So everything else being equal, a random sample with a higher mean is more likely to trigger the stopping rule than a random sample with a lower mean.

Because of the inherent randomness in the BREUT valuation process, some of the CE sample means will be lower and some will be higher the underlying distribution's mean CE. But the higher CE sample means result in lower coefficients

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<sup>2</sup> $CV(\{8; 10\}) \approx 1.4/9 \approx 0.16 > 0.13 = 1.4/11 \approx CV(\{10; 12\})$

of variation. The stopping rule tends to end the process for lower coefficients of variation and is therefore more likely to end it for a CE sample with a higher mean. So the stopping rule is more likely to settle on higher valuation samples as a final valuation.

So with respect to the mean of the underlying distribution, the sampling process for a valuation will stop *earlier* if CE sample means are *higher*. Similarly, the process will go on for *longer* if CE sample means are *smaller*. Thereby, CE samples with lower means are more likely to be selected for additional sampling. On average, they then increase their sample mean again as the sample mean regresses back to the underlying mean.

Lotteries that are riskier will have a higher variance in CE values. The trivial case of a low-risk lottery is a sure amount. Assume the mean risk-averse RP model  $u(x) = x^{1-r}$  with  $r \in [-0.3; 0.7]$ . A certain payoff of £10 will always generate a CE of £10. A 50% chance of £20 has the same expected value but will generate CEs from £1.98 to £11.73<sup>3</sup>. Even more extreme, CEs for a 25% chance of £40 with the same expected value range from £0.39 to £13.77. So riskier lotteries have a higher variance in their CE distribution and thereby generate CE samples with means that are more likely to be further from the mean of the underlying distribution.

Because of this, riskier lotteries more frequently create situations where the stopping rule settles on especially high valuations. Also, they will be overvalued more strongly relative to the underlying mean of their CE distribution because CE samples are further from it on average. The driving force for this effect is the coefficient of variation as the basis for the stopping rule. Therefore, the reliance of the stopping rule on the coefficient of variation causes riskier lotteries to be valued higher than the underlying CE distribution's mean more frequently and more strongly than safer lotteries.

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<sup>3</sup>With a majority of CEs below £10 because the majority of  $r$  parameters are above zero and therefore risk-averse.

### 4.3.2 Variables used and computed by the Model

The valuation model uses the same parameters that apply to BREUT for both choices and valuations. Choice probabilities are identical to BREUT but the valuation model extends predictions to also cover valuations. In addition, section 4.4 will explain how this can also apply to MSoP valuations. Furthermore, section 4.4.2 also shows how effects from consistency-seeking behaviour can be added in the form of a restriction on the CE distribution.

#### 4.3.2.1 Input Variables

Both BREUT choices and valuations are computed from independent random draws of CE values for the lotteries considered. These are generated by the same underlying distribution of CE values that are generated by a distribution of CRRA utility functions. An additional individual-specific parameter is the measure of a change in confidence  $d$ . This parameter is identical for choices and valuations, but is treated differently according to either a choice or a valuation procedure.

#### 4.3.2.2 Output Variables

##### **BREUT Choices**

The core model is BREUT, which already generates binary choice probabilities, either between lotteries or between lotteries and sure amounts. But in its adaption for the valuation model, BREUT choices also generate a sample of CE values that can be used in a subsequent valuation. In addition, BREUT choices allow us to compute an SI point for each lottery.

##### **Valuations**

The model generates a distribution of valuations,  $CE_K$ , equal to the means of each final CE sample the valuation is based on.

### Valuation Sample Sizes

Each generated valuation's CE sample has a specific sample size  $K$  at which the process stopped. Sample size can also be interpreted as a measure of deliberation time for a valuation task. The higher the sample size, the longer a participant would have needed to complete the task, *ceteris paribus*.

Directly related to the sample size, the confidence threshold can also be computed.  $d(k - 1)$  shows how small the coefficient of variation of the CE sample needed to be in order for the valuation process to stop.

#### 4.3.3 Model Simulation: Choices and Valuations

BREUT was simulated 10,000 times for each decision problem with the following core function for generating CE samples:

$$u(x) = x^{1-r} \quad \text{where } r \sim (\text{beta}(3, 3) \cdot \beta + (\alpha - \beta/2)) \quad \text{where } \alpha = 0.185 \beta = 1.0$$

The decrease in confidence per step was set to  $d = 0.1$ . These parameters are identical to the ones used by Navarro-Martinez et al. (2017) except for  $\alpha$ . The parameter  $\alpha$  was set to 0.185 instead of 0.35 as it will give an equal choice probability between the P-Bet and \$-Bet, which better illustrates the properties of the model. As in the last chapter, an 80% chance of winning £12 was used as a P-Bet and a 25% chance of winning £50 was used as the \$-Bet. These lotteries were also used in the experiment reported in chapter 5, which tests model predictions.

The *beta* distribution  $\text{beta}(3, 3)$  creates the shape of the  $r$  distribution;  $\beta$  is equal to its range and  $\alpha$  determines where the midpoint of the range lies.  $\text{beta}(3, 3)$  creates a symmetrical bell curve over  $[0; 1]$  with  $\text{mean} = \text{median} = 0.5$ .  $\alpha$  then re-centres this distribution around the mean  $\alpha$  and  $\beta$  adjusts its range to equal  $\beta$ .

All CE values are generated from the same  $r$  distribution, regardless of the lottery type. Therefore, DMs are assumed to have a stable underlying *distribution of risk aversion parameters*. Note that this symmetric  $r$  distribution results in an asymmetric distribution of corresponding CE values (see figure 4.1). As  $(1 - r)$



is in the exponent of the utility function  $u(x) = x^{1-r}$ , linear changes in  $r$  result in non-linear changes of the corresponding CE values. See section A.1 in the Appendix for an overview.

An 80% chance of winning £12 (expected value of £9.60) was used as the P-Bet and a 25% chance of winning £50 (expected value of £12.50) was used as the \$-Bet. Note that all possible values for  $r$  generate CE values that are strictly greater than zero and that the variance of CE values is higher for the \$-Bet, as shown in figure 4.1. While the underlying  $r$  distribution for P-Bet and \$-Bet CEs is identical, the transformation of a risk parameter into a CE causes the resulting CE distribution to be different across the lotteries in range, mean, and shape.

The resulting coefficients of variation that are estimated at different step sizes are illustrated by a simulation of 1.000 coefficients per sample size in figure 4.2. This figure shows the average coefficient of variation at a particular step, separately for the P-Bet and the \$-Bet. Note that the coefficient of variation at each step is also dependent on previous steps. A CE sample is carried over into an additional step and increased by another CE only if the coefficient of variation was above the confidence threshold at the previous step. Samples with lower coefficients of variation lead to early stopping in the process while higher coefficients of variation lead the process to keep sampling for longer.

Figure 4.2 illustrates how this affects mean coefficients of variations at different steps. The figure shows average coefficients of variation at each step  $k$ , with whiskers denoting 5% and 95% quantiles (meaning that 95% of observations fall within the whisker range). The dotted line shows the confidence threshold at step  $k$ . Note that steps for  $k > 3$  are based on the CE samples with a coefficient of variation that was *above* the confidence threshold at the previous step. If it was not, the process would have stopped at the previous step. E.g. at step  $k = 3$ , all samples had a coefficient of variation above  $d = 0.1$  at the previous step  $k = 2$ . Also, the valuation process stops for all samples that have a coefficient of variation below the threshold, i.e. the dotted line at the respective step.

So because of the stopping rule, only CE samples with a sufficiently high

coefficient of variation are carried on to a subsequent step. Also, if most of the coefficients of variation are below the confidence threshold, only a minority will get carried over to the next step. This selection due to the stopping rule only leaves samples with higher and higher coefficients of variation to remain in the process for later steps. This also explains why higher sample sizes are possible for the \$-Bet: the coefficients of variation are higher, so the process can carry on for longer.

Figure 4.1: BREUT: Underlying CE sample distributions

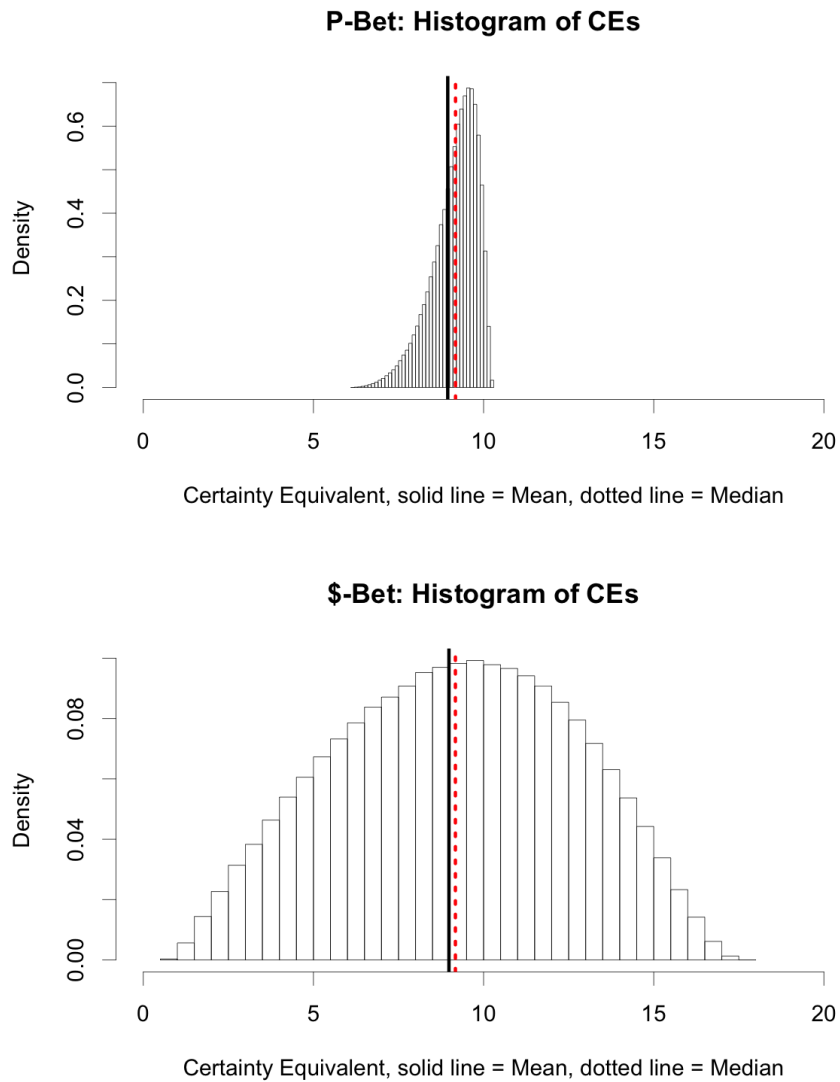
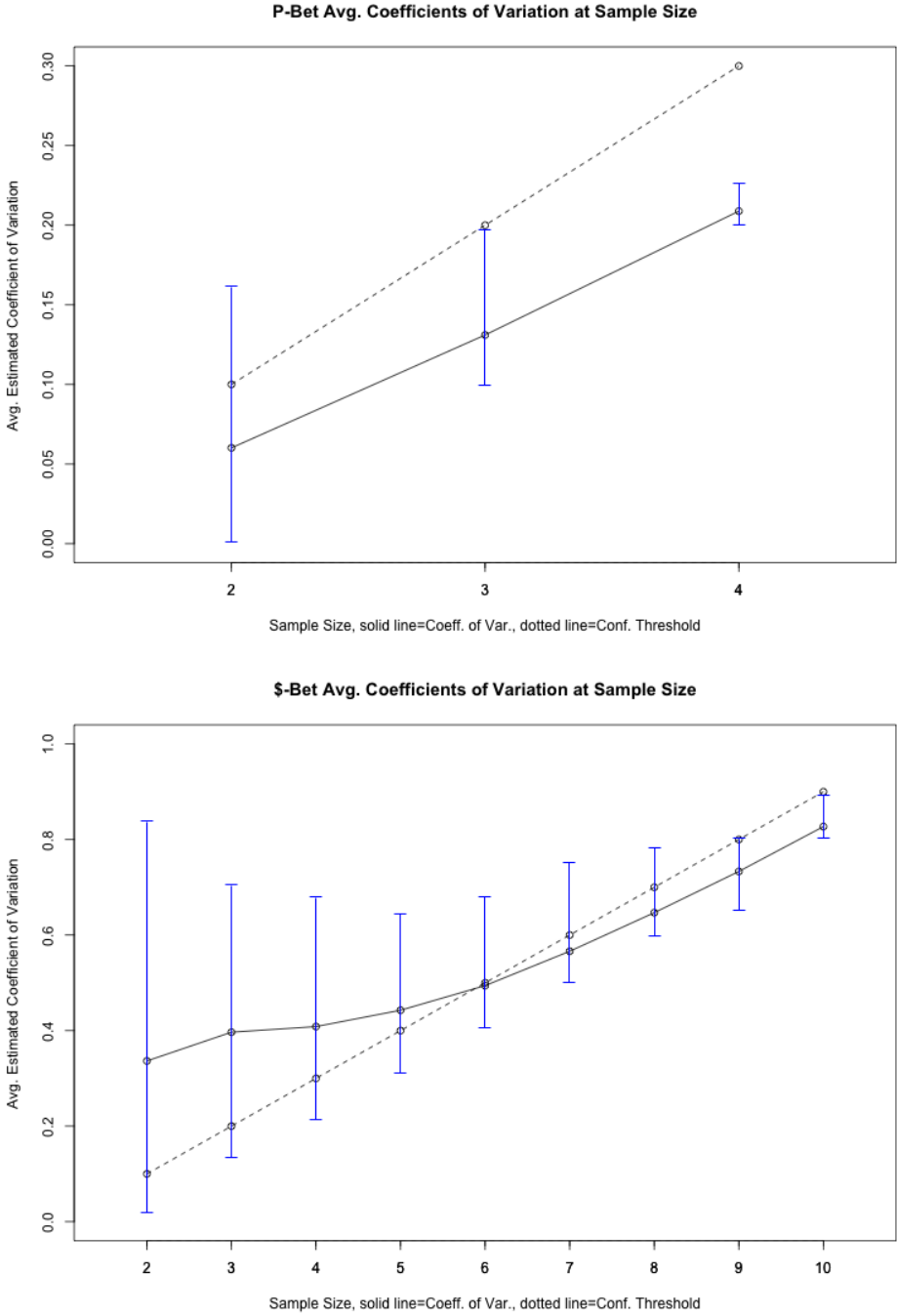


Figure 4.2: BREUT: Mean Coefficients of Variation generated per Step by the BREUT Valuation Process



Note: Whiskers indicate 5% to 95% quantiles.

#### 4.3.3.1 Choices

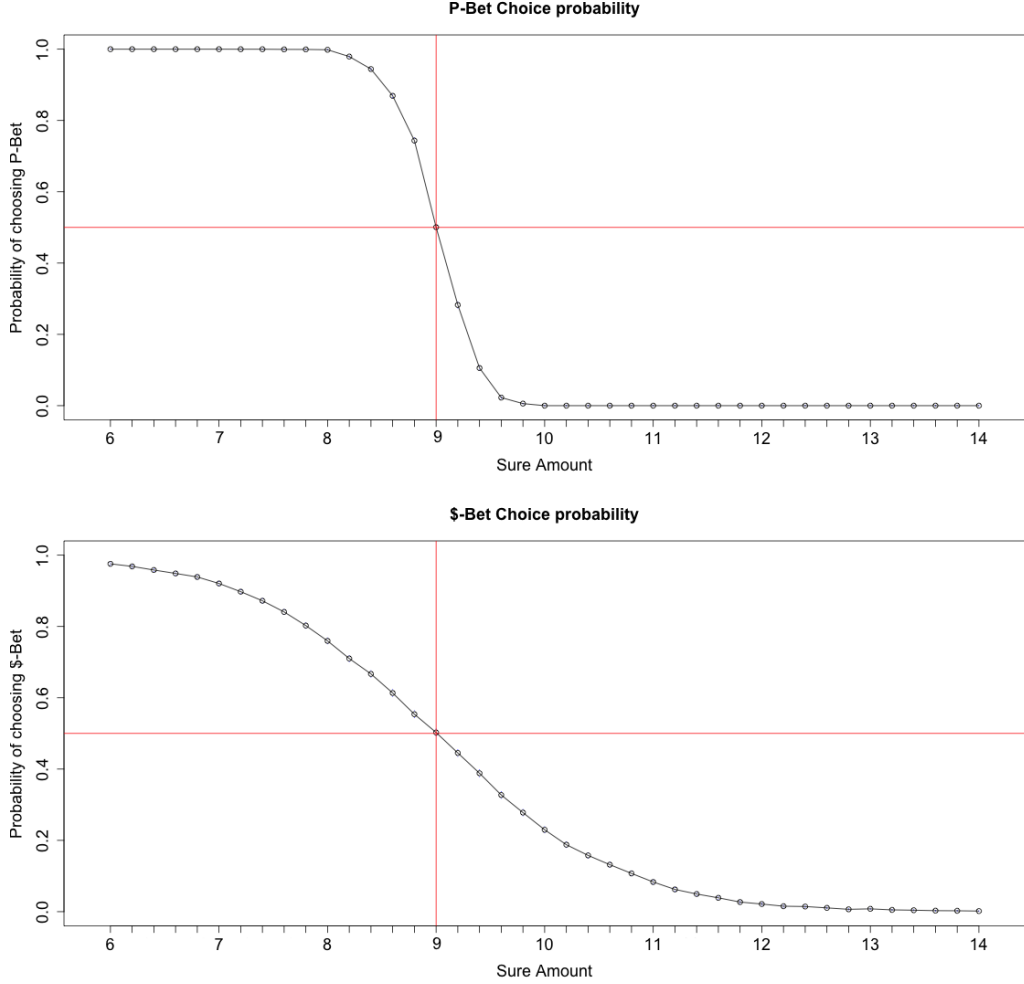
The parameters lead to a 50% BREUT choice probability between P-Bet and \$-Bet. Also, the P-Bet's and the \$-Bet's SI points both lie at £9.00<sup>4</sup>. BREUT predicts a steeper decrease in choice probabilities near the SI point for the P-Bet than for the \$-Bet. See figure 4.3 for mean choice probabilities against various sure amounts. Navarro-Martinez et al.'s (2017) parameters cause a choice probabilities that result in a curve are far closer to a deterministic step function for the P-Bet than for the \$-Bet.

Altering the  $\alpha, \beta$  and  $d$  parameters changes average preferences in choices between lotteries and sure amounts, as summarised by Navarro-Martinez et al. (2017) and will be explained later in the description of comparative statics. Navarro-Martinez et al. (2017) describe the impact of parameters in more detail and also for different binary choice problems. But this simulation is limited to their effect on valuations and preference reversals and therefore excludes this description.

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<sup>4</sup>Note that the SI point is not necessarily equal to the mean or median of the underlying CE distributions. Navarro-Martinez et al. (2017) describe that for  $d = 0.5$ , the SI point is always equal to the median of the CE distribution. As  $d$  approaches zero, the lottery's SI point moves from the underlying median CE to the underlying mean CE.

Figure 4.3: BREUT: Lottery Choice Probabilities over increasing Sure Amounts (£0.20 increments)



#### 4.3.3.2 Valuations

See the following table 4.1 for an overview of key model predictions. “%  $P \succ \$$ ” lists the percentage of choices in which BREUT chose the P-Bet over the \$-Bet and “%  $CE_{PK} > CE_{\$K}$ ” lists the percentage of P-Bet valuations that were higher than \$-Bet valuations. BREUT generates a 50% choice probability in favour of the P-Bet, and BREUT’s valuation model values the P-Bet higher than the \$-Bet only in 46% of cases. Still, this underpredicts the strong effects of the preference reversal

phenomenon, e.g. as observed by Grether and Plott (1979).

Table 4.1: Results for parameters  $\alpha = 0.185$ ,  $\beta = 1.00$ ,  $d = 10\%$ ,  $\text{beta}(3, 3)$

**P-Bet**

$\%P \succ \$$	$\%CE_{PK} > CE_{\$K}$	$\overline{CE_K}$	$\text{var}(CE_K)$	$\bar{K}$	$\%CE_K > \overline{\text{Core } CE_s}$	$\%CE_K > \text{SI Point}$
50%	46%	9.03	0.19	2.18	56%	54%

**\$-Bet**

$\%P \succ \$$	$\%CE_{PK} > CE_{\$K}$	$\overline{CE_K}$	$\text{var}(CE_K)$	$\bar{K}$	$\%CE_K > \overline{\text{Core } CE_s}$	$\%CE_K > \text{SI Point}$
50%	46%	9.29	3.58	4.50	53%	58%

“ $\overline{CE_K}$ ” shows the mean valuation and “ $\text{var}(CE_K)$ ” the variance of valuations for the respective lottery. The mean P-Bet valuation is £9.03 and the mean \$-Bet valuation is £9.29. \$-Bet valuations show a larger variance of 3.58 compared to P-Bet valuations (0.19) and have the median above the P-Bet’s median valuation. Because of this, the \$-Bet is valued higher than the P-Bet in 54% of the cases, which constitutes a preference reversal because of the \$-Bet is chosen equally often as the P-Bet (“ $\%P \succ \$$ ”=50%).

“ $\% CE_K > \overline{\text{Core } CE_s}$ ” lists the share of valuations that were higher than the mean of the underlying CE distribution. While the average P-Bet valuation surpasses the mean of its underlying CE distribution by less than 1%, the average \$-Bet valuation surpasses it by 3%. Accordingly, average \$-Bet valuations are higher than average P-Bet valuations but only to a small extent. And because choices are based on the same underlying CE distribution, it follows that the predicted effect strength of the preference reversal phenomenon is weak as well. Nevertheless, this demonstrates an interesting case where procedure variance is violated despite both procedures being built on the same set of underlying transitive EUT functions.

Although not all predictions in table 4.1 will be observable in an experiment, it illustrates an important property of the valuation model. As explained in section 4.3.1, this is the result of the stopping rule favouring valuations above the mean

of the underlying distribution. The higher the variance relative to the mean of a lottery's underlying CE distribution, the higher the extent of this effect.

This can be illustrated by the following example: Consider a sure amount of £10. Also consider that any valuation above the mean of the underlying CE distribution is an “overvaluation”. Now all CEs for a sure amount of £10 will equal £10, therefore a CE sample of a sure amount has zero variance and no overvaluation occurs. Relative to the mean of its underlying CE distribution, the P-Bet has a higher variance and on average results in an overvaluation. The \$-Bet has the highest variance (relative to the mean of its underlying distribution) and also the highest average extent of overvaluation. Therefore the variance of a lottery is the main driver to its overvaluation (compared to the mean of the underlying CE distribution).

“ $CE_K > SI\ Point$ ” shows the share of lottery valuations that are higher than the lottery's SI point. Both lotteries are more frequently overvalued relative to their SI point. This implies that the DM overvalues lotteries relative to their SI points as well. But the \$-Bet (58%) more so than the P-Bet (54%).

The average sample size “ $\bar{K}$ ” used for a lottery valuation is 2.18 for the P-Bet and 4.50 for the \$-Bet. Figure 4.5 shows histograms for the sample sizes for both lotteries. While most P-Bet valuations have a sample size of 2, most \$-Bet valuations have a sample size of 5. A larger sample size will lead to a more exact estimation of the mean of the underlying CE distribution (see section 4.3.3.3 for a discussion of the marginal effects of different sample sizes). Nevertheless, the average \$-Bet valuation is more frequently overvalued relative to the SI point. This overvaluation of \$-Bets leads to a higher average \$-Bet preference in valuations when comparing it to choices.

For a histogram of the lottery valuations, see figure 4.4 . Compared to the underlying distribution of CEs in figure 4.1, the distribution of final valuations is more narrow, because it averages from this CE distribution.

Figure 4.4: BREUT: Lottery Valuations

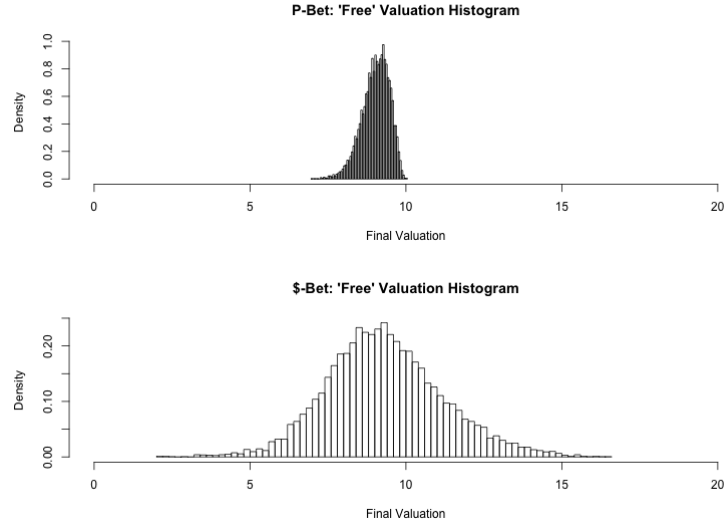
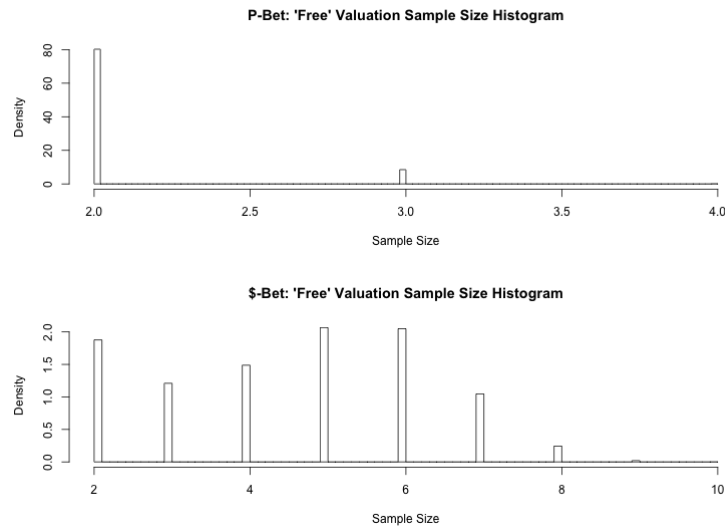


Figure 4.5: BREUT: Lottery Sample Sizes



## Preference Reversals

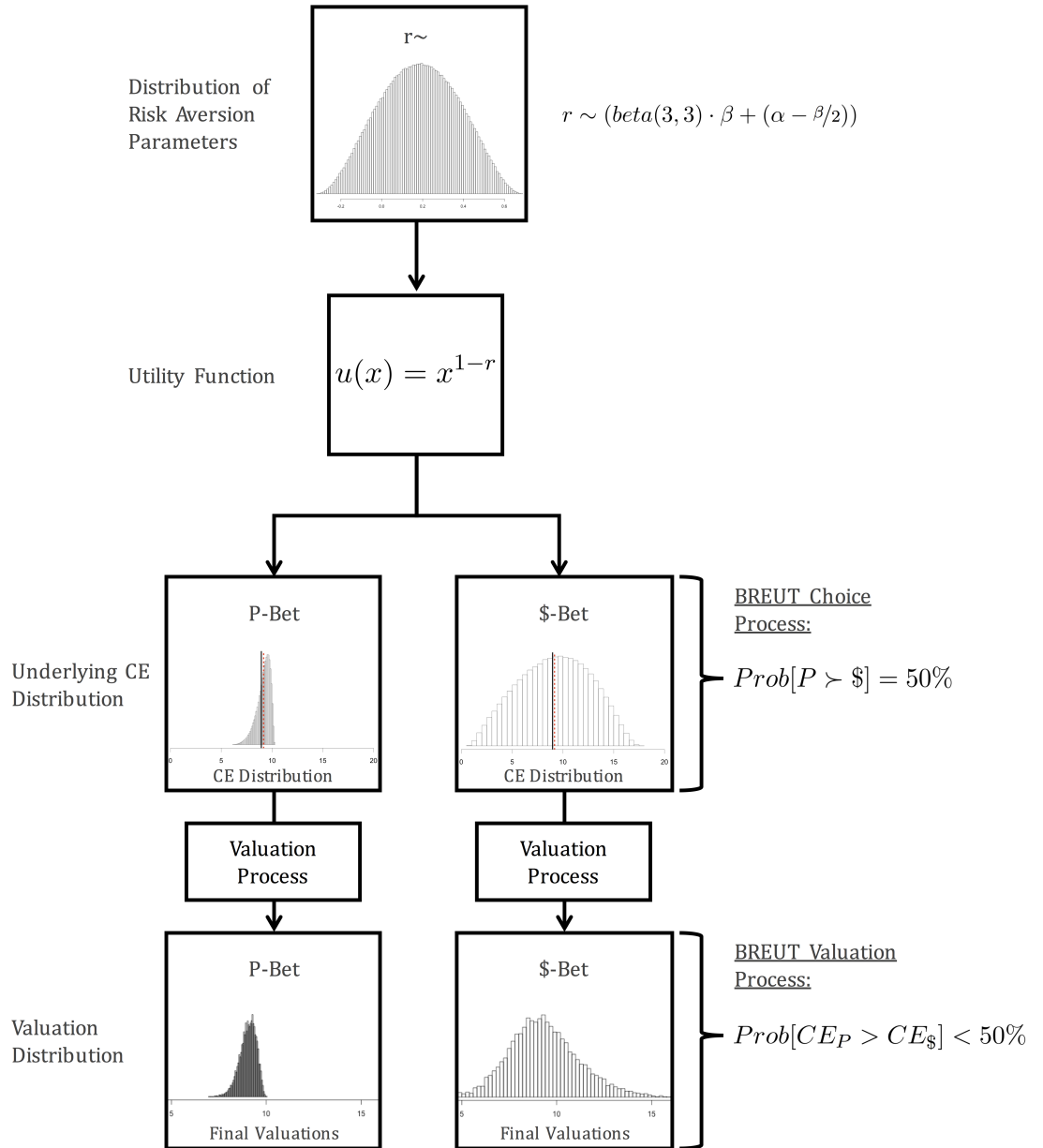
The model systematically predicts valuations that are above the SI points of the lotteries. This effect is stronger for the high-variance \$-Bet, even though \$-Bet valuations have a larger average sample size.



When sampling unusually high CEs, the valuation process stops earlier than on average, increasing the likelihood of prematurely generating a final valuation above the mean of the underlying distribution of CEs. This is because the stopping rule is more likely to be triggered for a sample with a higher mean, given the same variance (see section 4.3.1, “Component 3”). Vice versa, sampling unusually low CEs leads the process to go on for longer, which weakens the effect of the low CEs on the entire sample mean. This introduces an overvaluation effect in lottery valuations.

This effect increases in the variance of the CE distribution as this causes more frequent and more extreme instances of especially high or low CE sample means. The \$-Bet has a larger variance in payoffs than the P-Bet, which generates a larger variance in the \$-Bet’s underlying CE distribution. Despite the larger average sample size of its valuations, this leads the \$-Bet to be overvalued more (relative to its SI point as well as the underlying CE distribution) than the P-Bet. Therefore, parameters exist where the valuation model predicts \$-Bet valuations above P-Bet valuations more frequently than the BREUT core model chooses the \$-Bet over the P-Bet. Figure 4.6 illustrates this mechanism with a flowchart.

Figure 4.6: Flowchart to illustrate how the BREUT Valuation Model predicts the Preference Reversal Phenomenon



### **Comparative Statics: Risk Aversion $\alpha$**

Table 4.2 shows the effects of altering the median risk aversion parameter  $\alpha$  on predictions. As  $\alpha$  increases, the average preference for the less risky P-Bet over the \$-Bet increases as well. Still, the extent of overvaluation compared to lottery's SI point is more frequent for the \$-Bet for the majority of parameters used (as evident through the %  $CE_K > \text{SI Point}$  ratios). Also, \$-Bet valuations require larger sample sizes and show a higher variance for all parameters used.

Table 4.2: Changing risk aversion parameter  $\alpha$  while  $\beta = 1.00$ ,  $d = 10\%$ ,  $beta(3, 3)$

P-Bet							
$\alpha$	% $P \succ \$$	% $CE_{PK} > CE_{\$K}$	$\overline{CE_K}$	$var(CE_K)$	$\overline{K}$	% $CE_K > Core\ CE_s$	% $CE_K > SI\ Point$
-0.25	0%	0%	9.99	0.04	2.00	52%	51%
-0.10	0%	0%	9.74	0.07	2.01	53%	52%
0	1%	1%	9.53	0.10	2.05	54%	51%
0.10	14%	22%	9.28	0.14	2.10	56%	52%
<b>0.185</b>	<b>50%</b>	<b>46%</b>	<b>9.03</b>	<b>0.19</b>	<b>2.18</b>	<b>56%</b>	<b>54%</b>
0.25	80%	66%	8.80	0.26	2.27	58%	55%
0.30	93%	79%	8.58	0.32	2.35	57%	54%
0.35	98%	87%	8.34	0.40	2.47	58%	51%
0.50	100%	98%	7.44	0.79	3.18	61%	53%

\$-Bet							
$\alpha$	% $P \succ \$$	% $CE_{PK} > CE_{\$K}$	$\overline{CE_K}$	$var(CE_K)$	$\overline{K}$	% $CE_K > Core\ CE_s$	% $CE_K > SI\ Point$
-0.25	0%	0%	16.42	3.22	2.82	53%	52%
-0.10	0%	0%	14.19	3.62	3.16	54%	52%
0	1%	1%	12.49	3.65	3.53	53%	52%
0.10	14%	22%	10.78	3.80	3.99	53%	53%
<b>0.185</b>	<b>50%</b>	<b>46%</b>	<b>9.29</b>	<b>3.58</b>	<b>4.50</b>	<b>53%</b>	<b>58%</b>
0.25	80%	66%	8.13	3.29	5.07	54%	55%
0.30	93%	79%	7.22	3.08	5.56	53%	54%
0.35	98%	87%	6.33	2.78	6.17	52%	55%
0.50	100%	98%	3.84	1.59	8.65	51%	59%

### Comparative Statics: Risk Aversion Parameter Range $\beta$

Table 4.3 shows predictions for different  $\beta$  values. Increasing  $\beta$  widens the range of the  $r$  and CE distributions. This is equivalent to increasing noise in the valuation process. This results in larger sample sizes that are needed for valuations and a higher variance in valuations (again, both of these are always larger for the \$-Bet). The size of the preference reversal, the difference between “%  $P \succ \$$ ” and “%  $CE_P > CE_\$$ ” is also larger for higher  $\beta$  values.

Table 4.3: Changing the range of risk aversion parameters  $\beta$  while  $\alpha = 0.185$ ,  $d = 10\%$ ,  $\beta_{eta}(3, 3)$

P-Bet							
$\beta$	% $P > \$$	% $CE_{PK} > CE_{\$K}$	$CE_K$	$var(CE_K)$	$\bar{K}$	% $CE_K > Core\ CE_s$	% $CE_K > SI\ Point$
0.25	50%	50%	9.11	0.01	2.00	51%	51%
0.40	50%	49%	9.11	0.03	2.00	52%	51%
0.55	50%	48%	9.09	0.06	2.00	53%	51%
0.70	50%	47%	9.07	0.09	2.04	54%	52%
0.85	50%	46%	9.04	0.14	2.11	55%	53%
<b>1.00</b>	<b>50%</b>	<b>46%</b>	<b>9.03</b>	<b>0.19</b>	<b>2.18</b>	<b>56%</b>	<b>54%</b>
1.15	49%	45%	8.99	0.26	2.26	58%	53%
1.30	49%	45%	8.96	0.35	2.36	60%	51%

\$-Bet

$\beta$	% $P > \$$	% $CE_{PK} > CE_{\$K}$	$CE_K$	$var(CE_K)$	$\bar{K}$	% $CE_K > Core\ CE_s$	% $CE_K > SI\ Point$
0.25	50%	50%	9.12	0.38	2.33	50%	50%
0.40	50%	49%	9.13	0.91	2.72	51%	51%
0.55	50%	48%	9.14	1.50	3.14	51%	51%
0.70	50%	47%	9.19	2.17	3.60	52%	52%
0.85	50%	46%	9.22	2.82	4.05	52%	54%
<b>1.00</b>	<b>50%</b>	<b>46%</b>	<b>9.29</b>	<b>3.58</b>	<b>4.50</b>	<b>53%</b>	<b>58%</b>
1.15	49%	45%	9.34	4.31	5.00	54%	54%
1.30	49%	45%	9.39	4.89	5.46	54%	53%

### Comparative Statics: Confidence Level Reduction Rate $d$

Table 4.4 shows how different values for  $d$  influence predictions. As  $d$  determines the required level of confidence for each step, it is a proxy for the “impatience” of the DM both in the valuation and in the choice process. Especially high  $d$  values result in the lowest sample sizes and the highest variance in valuations. Especially low  $d$  values, signifying a particularly high level of required confidence and therefore a more patient DM, result in higher sample sizes and less variance in valuations.

But the extent of preference reversals, the difference between “%  $P \succ \$$ ” and “%  $CE_P > CE_\$$ ”, is not strictly increasing in  $d$ . As  $d$  increases, more and more valuation processes hit the stopping rule with the first sample of two CE values.

For especially high values of  $d$ , the stopping rule is more likely to stop the process for any coefficient of variation. As a result, valuations are more likely to be the result of a “truly random” sample of two CE values because the process is always stopped at  $k = 2$ . Valuations are therefore more varied but also not likely to be stopped early if they are higher. Only when the stopping rule prolongs the process for lower valuation samples, does it cause a bias (note that this dynamic is different for MSOP values, explained in section 4.4.1). If the stopping rule distinguishes between CE samples, it means that valuation samples above the SI point result in more final valuations, increasing the number of overvaluations.

But conversely, especially low values of  $d$  also weaken the preference reversal because they increase the average CE sample size for all valuations. The higher sample sizes are, the weaker the overvaluation of the \$-Bet becomes, resulting in fewer preference reversals. So the effect of higher sample sizes outweighs the effect of the stopping rule.

As the sample size  $K$  can also be interpreted as a proxy for reaction times, altering the  $d$  parameter also illustrates changes in reaction times. For higher  $d$  values, the DM samples quickly and should therefore also react quickly with an imprecise valuation. Conversely, a DM with a low  $d$  takes longer but then reports CEs with a lower variance across repetitions.

Table 4.4: Changing confidence level decrease parameter  $d$  while  $\alpha = 0.185$ ,  $\beta = 1.00$ ,  $beta(3, 3)$

P-Bet							
$d$	% $P > \$$	% $CE_{PK} > CE_{\$K}$	$CE_K$	$var(CE_K)$	$\bar{K}$	% $CE_K > Core\ CE_s$	% $CE_K > SI\ Point$
0.01	49%	49%	9.06	0.08	7.16	57%	54%
0.05	49%	47%	9.04	0.17	2.61	56%	52%
<b>0.10</b>	<b>50%</b>	<b>46%</b>	<b>9.03</b>	<b>0.19</b>	<b>2.18</b>	<b>56%</b>	<b>54%</b>
0.15	50%	46%	9.01	0.21	2.06	56%	52%
0.20	50%	47%	9.01	0.21	2.02	55%	51%
0.30	50%	47%	9.00	0.22	2.00	54%	50%
0.40	49%	47%	9.00	0.22	2.00	55%	50%
0.50	50%	48%	9.00	0.21	2.00	55%	50%

\$-Bet							
$d$	% $P > \$$	% $CE_{PK} > CE_{\$K}$	$CE_K$	$var(CE_K)$	$\bar{K}$	% $CE_K > Core\ CE_s$	% $CE_K > SI\ Point$
0.01	49%	49%	9.10	0.49	38.91	52%	52%
0.05	49%	47%	9.26	2.37	8.02	53%	54%
<b>0.10</b>	<b>50%</b>	<b>46%</b>	<b>9.29</b>	<b>3.58</b>	<b>4.50</b>	<b>53%</b>	<b>58%</b>
0.15	50%	46%	9.27	4.23	3.51	53%	53%
0.20	50%	47%	9.25	4.72	3.02	53%	53%
0.30	50%	47%	9.21	5.30	2.59	52%	52%
0.40	49%	47%	9.17	5.66	2.36	52%	51%
0.50	50%	48%	9.14	5.72	2.25	52%	50%



#### 4.3.3.3 Deliberation Time and Valuations

See figures 4.7 and 4.8 for violin plots of final P-Bet and \$-Bet valuation distributions, separated by sample sizes. Final valuations are generated from parameters of the baseline case ( $\alpha = 0.185$ ,  $\beta = 1.00$ ,  $d = 0.1$ ,  $\text{beta}(3, 3)$ ). Plotted is the lottery overvaluation. I.e., the lottery valuation minus the SI point of the respective lottery.

Note that absolute sample sizes might not be comparable between choice and valuation tasks. The BREUT choice process uses different operations to compare CEs and has a different stopping rule. The same might apply to participants in an experimental context, where mental processes for choices as well as executions for choices might be quicker than for valuations.

Experimental evidence considering this will be explored in chapter 6. An experimental test of this would be to measure if individually-longer deliberation times for the same lotteries do have a negative effect on valuations. In an experiment, this would mean that average valuations are above the participant's SI point but less so for individually-longer deliberation times.

E.g., a participant would not only be quicker in comparing between options but also quicker in selecting a preferred option than in reporting a valuation. So while sample sizes might be a good proxy for reaction times in an experiment, a given sample size within a choice task might correspond to a different reaction time than for a valuation task. But it makes sense to compare reaction times within *valuation* tasks only. Three dimensions stand out and can be explained by the model characteristics:

**1) The larger the sample, the smaller the valuation of a lottery.** For the smallest sample size, the majority of P-Bet valuations is above the SI point. But for the largest sample size, the majority of P-Bet valuations is below it. The same applies to the \$-Bet. So the shorter the deliberation time of a lottery valuation, the greater the likelihood that the lottery is overvalued. This is because the stopping rule is biased to stop for CE sample means that are above the underlying distribution's mean (see section 4.3.1). Therefore, above-mean CE samples are likely to be stopped

and reported early. Below-mean CE samples are likely to carry on, stopping late in the sampling process. And since fewer valuations are below the SI point, this results in an average overvaluation of the lottery compared to the SI point.

In addition, this effect is stronger for the \$-Bet. Therefore, early \$-Bet valuations are more likely to lie above early P-Bet valuations. For later valuations, the reverse applies with more P-Bet valuations above \$-Bet valuations.

**2) The difference between valuations and SI points is initially positive and eventually becomes negative with increasing sample size.** This also follows from the stopping rule. Since below-SI CE samples carry on sampling for longer, samples that initially have a low sample mean carry on and eventually result in higher sample sizes. While below-SI samples trigger a later finish, the corresponding above-SI samples trigger an earlier finish. This leads to a divergence in sample size between valuations that are biased upwards and those that are biased downwards (compared to the mean of the underlying CE distribution). For repeated valuations of the same lottery, valuations that are stopped earlier have a smaller sample, so their upward bias is stronger than the downward bias of valuations that stop later. This is because the bias is weakened by a larger sample size. So valuations that stop later have a larger sample size, resulting in a smaller (downward) bias.

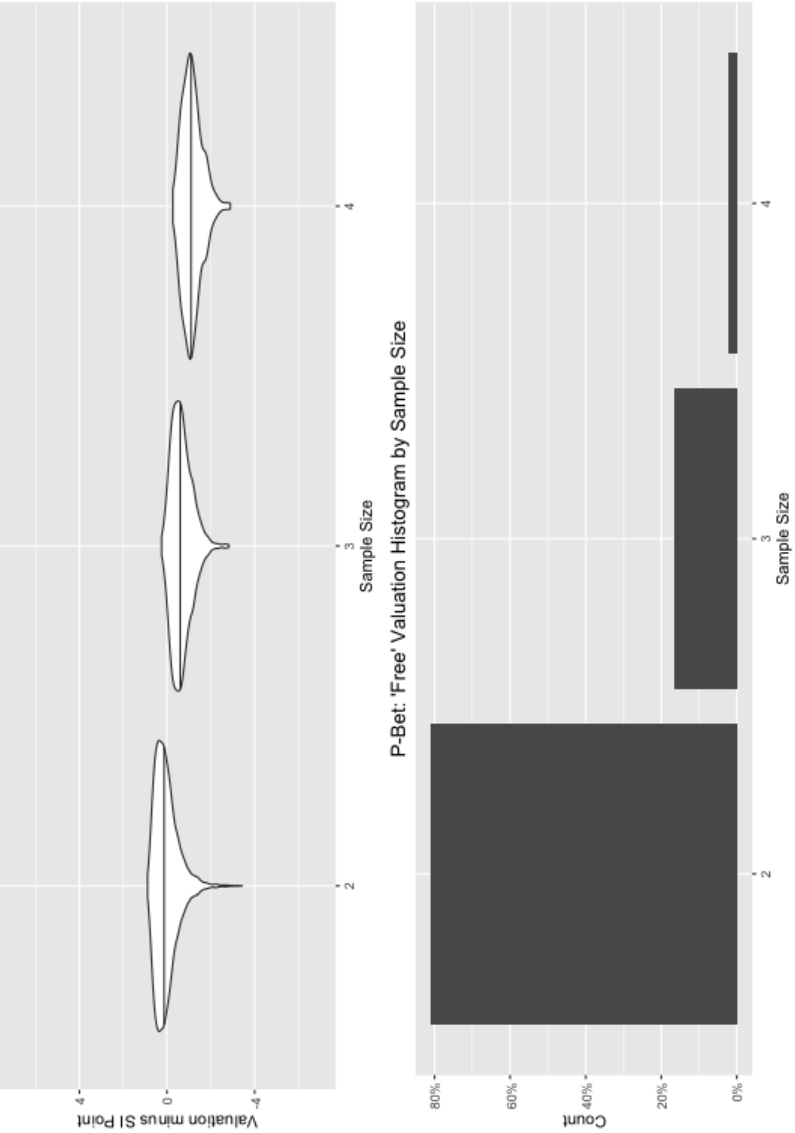
**3) P-Bet valuations are based on smaller samples than \$-Bet valuations.**

The mean P-Bet sample size of 2.19 is less than half as big as the mean \$-Bet sample size of 4.24. This fits in well with existing data, where valuations of safer lotteries typically take less time than valuations of riskier lotteries (e.g. Schkade and Johnson, 1989). Given that \$-Bet sample sizes are higher, it is surprising to see that median \$-Bet valuations are nevertheless higher than median P-Bet valuations (see subsection 4.3.3.2).

The explanation for this is that overvaluation not only depends on the sample size. The size of an initial overvaluation is also determined by the underlying CE distribution for the lottery. The \$-Bet has an underlying CE distribution that has

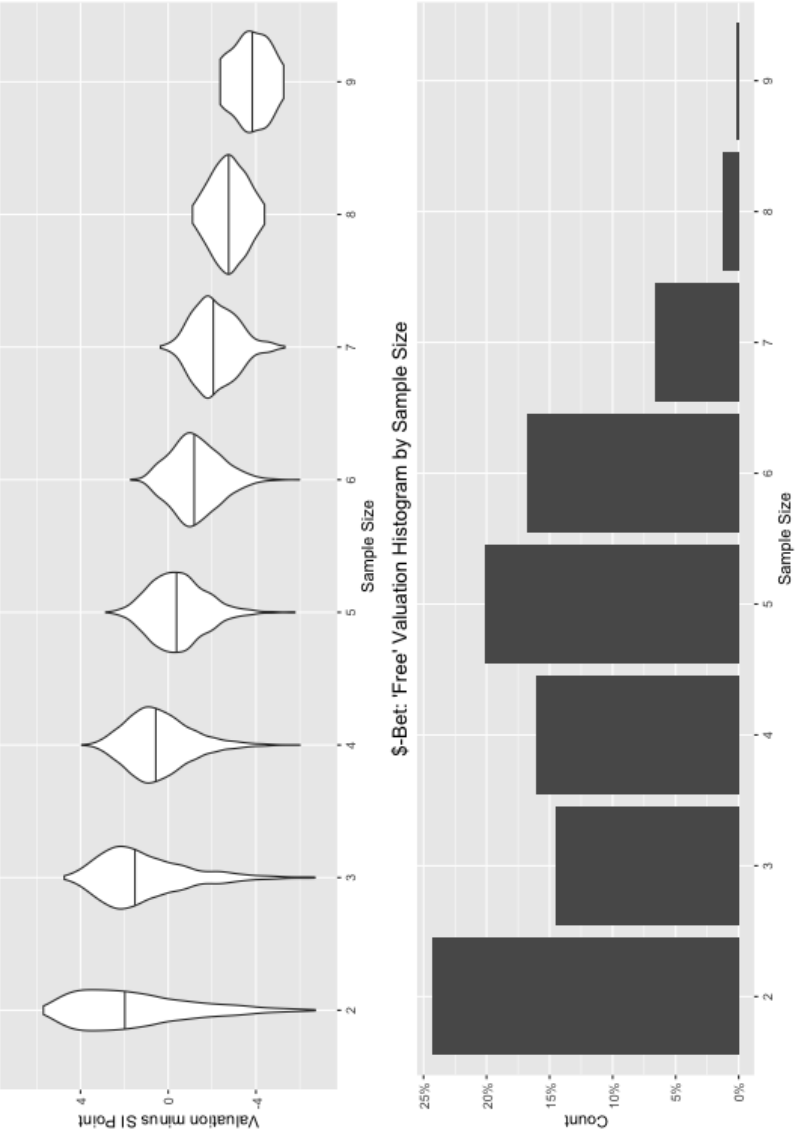
a larger range than that of the P-Bet (see start of section 4.3.3). So despite being curbed by larger sample sizes, \$-Bet valuations show such a strong overvaluation that they are nevertheless higher than P-Bet valuations. Thereby, the model captures both the observation of stronger overvaluations for \$-Bets than for P-Bets relative to SI points (Loomes and Pogrebna, 2016) as well as the effect that valuations take longer for \$-Bets than for P-Bets (Schkade and Johnson, 1989).

Figure 4.7: BREUT Valuation Model P-Bet: Lottery Valuations, their corresponding Number of Steps, and their frequencies



Note: Median values are marked by horizontal lines

Figure 4.8: BREUT Valuation Model \$-Bet: Lottery Valuations, their corresponding Number of Steps, and their frequencies



Note: Median values are marked by horizontal lines

#### 4.3.3.4 Issues with Predictions

While the BREUT valuation model predicts the preference reversal phenomenon, Grether and Plott (1979) reported a stronger effect. In their example, the *majority* of participants both choose the P-bet over the \$-Bet while also valuing the \$-Bet higher. Table 4.2 illustrates the impact of changing  $\alpha$  on the mismatch between the preference in choices for the P-Bet versus the preference for it in valuations (which equals “%  $P \succ \$$ ” minus “%  $CE_{PK} > CE_{\$K}$ ”). All displayed  $\alpha$  parameters that predict a majority of P-Bet choices result in only a small mismatch in line with a preference reversal from the P-Bet to the \$-Bet. Therefore, the model underpredicts the preference reversal phenomenon for that data set.

The BREUT valuation model can predict a preference reversal but at the cost of limiting the variability of choices. The parameters result in a relatively low choice variability for the P-Bet (as shown in figure 4.3). If variability in choices is increased by choosing a larger variance for the distribution of  $r$  parameters, the size of the preference reversal effect weakens. This keeps the effect size of the preference reversal small. Nevertheless, it consistently shows a preference reversal from a P-Bet preference in choices to an increased \$-Bet preference in valuations.

### 4.4 Extending BREUT to model MSoP Values

Sections 2.2.1 and 2.2.2 in the literature review already discussed the MSoP mismatch and choice-induced changes in preference. This section will now explore two applications of the BREUT valuation model to capture an MSoP elicitation process: Spill-over effects and consistency-seeking behaviour.

#### 4.4.1 Spill-Over Effects of Mental Evidence from Choice to MSoP Elicitation

##### 4.4.1.1 Applicable Theoretical Work

MSoP values can be regarded as a strength of preference that is quantified to provide a CE. And since the strength of a preference is closely related to a participants’

confidence in their preferences, this resembles Moore and Healy’s (2008) definition of “overprecision”, i.e. an overestimation of the precision of one’s own beliefs.

Overprecision could be interpreted to explain an MSoP mismatch from a mental sampling perspective (see section 2.5.1 for an overview of applicable mental sampling models). In Butler et al.’s (2014a) observation of an MSoP mismatch, participants state a strength of preference that is too high to match with their average preference in choices. If participants base their decision making and the resulting monetary strength of preference response on a sampling process of mental evidence, this MSoP overstatement could result out of overprecision.

If such a participant made a choice in favour of an option A over some other option B, it would have sampled evidence in favour of choice A. This evidence would imply some strength of preference greater than zero for option A, even if this participant is *on average* indifferent between the two options. But this will be true for *all* samples of mental evidence that lead the participant to choose A. Because if the sample had not favoured A, the participant would have chosen option B instead. Therefore, all evidence samples favour the option that has just been chosen and will therefore value option A on average higher when compared to any underlying distribution of mental evidence.

If someone were fully aware of the fact that their preferences are probabilistic, they would need to take this into account when estimating their MSoP. But participants in experiments might be restricted in their access to mental evidence. If their construction of an MSoP valuation is subject to limited access to or awareness of mental evidence in support of a different choice, they would be limited to using the mental evidence from the previous choice. And basing their MSoP on that choice could give a too high valuation of option A. Therefore, an MSoP more consistent with their *underlying* average preferences would need to be smaller than implied by the sample. But if they are subject to overconfidence in the form of overprecision, they might just base their MSoP on their mental sample as it is<sup>5</sup>. Note that the

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<sup>5</sup>Dubra (2004) discusses a comparable case with microeconomic agents that have a prior set of beliefs and search for wage offers. Dubra defines agents with a prior of higher wages than the underlying distribution as “optimistic” and models overconfidence by assuming that these agents

simpler RP model can also explain the MSoP mismatch in this way. Section 3.2 covers this in more detail, while the next section applies this logic to the BREUT valuation model.

The result is that participants treat their mental evidence base as unbiased from their underlying preferences even though it is not. Mental evidence from the previous choice “spills over” into the subsequent valuation process. It excludes at least enough mental evidence to make them choose the option for which they will estimate their MSoP. Therefore, their MSoP is contingent on the choice they just made and leads them to be overconfident in favour of the chosen option: This spill-over of mental evidence could then lead to the too high MSoP values observed by Butler et al. (2014a).

#### 4.4.1.2 Modelling Spill-Over Effects

In our specification, BREUT already generates a sample of CEs for both options during a binary choice. We can connect a BREUT choice to a BREUT valuation by assuming that the mental sample of CE values is still present if a choice is immediately followed by an MSoP elicitation. The CE values from the choice will then already be incorporated in the mental sample before the valuation process for the MSoP starts. These CE values will then influence the sample mean. This bears some resemblance to a model of confidence judgements developed by Pleskac and Busemeyer (2010). In their model, DMs use mental evidence that they generated during a judgement for a subsequent estimation of their confidence in their judgement. Applied to the BREUT valuation model, this logic would imply that DMs might use their mental evidence from a choice task for a subsequent valuation.

Consider a participant that makes a choice between a lottery and a sure amount at the SI point. If she chooses the lottery over the sure amount on a particular occasion, her CE sample from the choice must imply a lottery valuation *higher* than the sure amount. Therefore, the CE sample mean is higher than the SI point. This is similar to the case in the RP model (see section 3.2.2), only that the

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do not factor in the possibility that their prior set of beliefs might be too optimistic.



preference is based on a CE sample instead of a single CE.

As mental evidence spills over into the subsequent MSoP process, the CE sample from the choice is carried over into the CE sample for the MSoP valuation. In the MSoP phase, a BREUT valuation process starts anew. It can happen that with the first additional sample, the sampling process already stops at a threshold  $d$  because the sample's CV is already low enough. In this case, the spill-over effect would be comparable to the case in the RP model where just the CE that led to the choice is reported (chapter 3, section 3.2). But if the sample's coefficient of variation is high enough, the DM will carry on with the valuation process.

So in the lottery choice case, the DM starts off the MSoP valuation process with a CE sample that has a sample mean above the SI point. Therefore, the CE sample is biased upwards. As the BREUT DM does not adjust for the likelihood of having a biased sample, this produces an overconfidence in the form of overprecision. Thereby, a spill-over effect emerges and leads to a positive MSoP mismatch.

If average MSoP values were consistent with *choices*, average MSoP values would always imply a valuation equal to the sure amount at the SI point, regardless of the preceding choice. This implies that consistent MSoP values at the SI point should average to zero, regardless of the previous choice. But the spill-over of mental evidence in the form of the CE sample prevents that. It biases the subsequent valuation to support the choice that was just taken, not the *average* of choices.

## 4.4.2 Consistency-Seeking Behaviour in MSoP Elicitation

### 4.4.2.1 Applicable Theoretical Work

Another approach that could explain the MSoP mismatch is cognitive dissonance. Bem's (1967) self-perception theory formulates cognitive dissonance as the result of an imperfect introspective process. This makes it applicable to bounded rationality in order to predict behaviour in an economic experiment<sup>6</sup>. Bem postulates that "self-descriptive attitude statements can be based on a participant's observations of their

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<sup>6</sup>More applicable than Festinger's (1957) theory of cognitive dissonance, which models cognitive dissonance through an avoidance of dissonant attitudes without information search.

own overt behaviour and the external stimulus conditions under which it occurs.” (1967, p.185). Applied to Butler et al.’s (2014a) experiment, this would mean that a participant’s “attitude” towards a lottery in the form of a CE is the result of 1) past choices (that are remembered) and 2) the participant’s preference. This stands in contrast to standard economic theory, which dictates that only subjective utility governs individual behaviour.

Bem details how this can explain the phenomenon that participants rank objects’ desirability higher *after* having chosen them vs. *before* having chosen them. This bears the risk of confounding with endowment effects that might have been induced by having chosen an object. But cognitive dissonance can still exist in case of participants choosing but not obtaining objects. Therefore it is reasonable to test for its existence in valuation tasks.

Bem does not discuss any procedural details in a decision process and simply assumes that any DM is an observer of her past selves while taking decisions. This DM attributes certain attitudes to her past actions while mostly ignoring the decision process at the time of action. Applying this self-attribution to a process that uses sequential sampling of mental evidence could mean that any evidence accumulation is subject to motivated reasoning: evidence which is dissonant with previous actions runs the risk of being discarded in the mental process. This would imply that the available evidence for such a process is restricted in favour of “self-consistent” evidence.

The simplest model of cognitive dissonance would assume that any accumulated evidence *inconsistent* with known past actions is discarded by the DM. However, other specifications are possible and applicable, e.g. only assuming a propensity to ignore evidence or the option to override a previous decision in face of overwhelming evidence. But if the decision making process is biased towards using evidence that is self-consistent, it will result in more decisions that are self-consistent as well because inconsistent mental evidence is ignored. The result are decisions in favour of consistent actions over simple actions that reflect the underlying mental evidence without bias. I.e., DMs subject to cognitive dissonance will

exhibit consistency-seeking behaviour.

This could also result in a positive MSoP mismatch if DMs just ignore any evidence that speaks against their choice in an MSoP task. If a DM is subject to such cognitive dissonance effects and has chosen a lottery over £10, this DM will ignore any mental evidence that suggests a lottery valuation below £10. Therefore, the DM will act in a consistency-seeking way. The result is a valuation that relies on a subset of especially high-valuing evidence that produces higher valuations because of this cognitive dissonance. Symmetrically, choosing £10 over the lottery would only consider evidence for a valuation below £10, resulting in a lower valuation.

#### 4.4.2.2 Modelling Consistency-Seeking Behaviour

We assume that DMs do not remember past choices in their choice or valuation process unless they were just reminded of it. We will use the most simple assumption of consistency-seeking behaviour: DMs who face a display of a past choice will ignore any mental evidence inconsistent with that choice.

Following Pleskac and Busemeyer’s (2010) evidence on post-decisional processing of mental sampling, this “contrary” evidence could, however, be present in working memory if a choice has just been taken. Pleskac and Busemeyer (2010) also suggest that contrary mental information to a decision can be generated straight after a choice. Therefore, we assume that dissonant information will not be ignored in an MSoP task *straight after* a choice. The experiment reported in chapter 5 considers such a task (section 5.2.2). As described in the previous section, this leads to a biased mental evidence base. But if contrary mental evidence is carried over as well, cognitive dissonance (in its strong form of ignoring any inconsistent evidence) will not be present in these immediate MSoP tasks straight after a choice.

Therefore, we assume that cognitive dissonance effects will only occur if a choice *display* is immediately followed by an MSoP task, not the actual choice. The experiment reported in chapter 5 also considers such a task (section 5.4). So MSoP tasks after a choice display will be subject to cognitive dissonance while no spill-over effects are present.

Assume that a BREUT DM faces a display of her past binary choice and a prompt to state her MSoP. Using the BREUT valuation process, she will accumulate a CE sample for the valuation of their MSoP but will not have access to the previous sample that led to the choice. So all mental evidence will be new. If this DM avoids all cognitively dissonant mental evidence, she will discard any information that is dissonant with her decision. Her motivated reasoning only accumulates evidence that suggests a positive MSoP consistent with her choice. Therefore, a consistency-seeking accumulation process restricts the mental sample to only feature self-consistent mental evidence.

Since evidence in the model is accumulated from mental CE samples of the lottery, cognitive dissonance will rule out any CE values in conflict with a displayed choice. Therefore, cognitive dissonance affects the valuation process not by introducing a CE sample but by restricting the range of CE values that can be sampled. As the process removes the motivation for DMs to act inconsistently with their past choice, the result is consistency-seeking behaviour.

For instance, if a DM sees a choice display of having chosen a lottery over £10, cognitive dissonance in the model will lead her to ignore any CE sample smaller than £10. In an MSoP task based on that choice display, the DM will only sample CE values at or above £10. Since the final valuation is equal to the CE sample mean, this must result in a valuation above £10, always implying an MSoP above £0. The DM's MSoP values will result in a positive MSoP mismatch due to her consistency-seeking behaviour.

#### 4.4.3 Model Simulation: MSoP Values

BREUT was simulated with the same  $\alpha$ ,  $\beta$  and  $d$  parameters as in section 4.3.3, again 10,000 times for each decision problem with the same core function for generating CE samples. In this section, MSoP tasks are simulated instead of a direct lottery valuation. This is done for two settings:

First, for an MSoP task straight after a choice between a lottery and a sure amount. I.e., the DM chooses an option and then reports a lottery valuation. This

“immediate adjustment” is the setting for spill-over effects.

Second, for the same MSoP task but when the DM only receives information about her previous action in the choice. This “later adjustment” is the setting for consistency-seeking behaviour.

Note that these two effects could in theory both occur in the same setting. In an MSoP task right after a choice, a DM could also be subject to both effects. Consistency-seeking effects can also occur as an added effect in immediate MSoP tasks. But in this section, these two components are kept separate to make them easier to understand. The following sections show how both effects result in positive MSoP mismatches. Spill-over effects work through affecting the CE sample before the process starts. Consistency-seeking behaviour works by affecting what CE values are sampled during the valuation process. So the resulting effects can just be added on to each other in case of assuming that both occur in an immediate MSoP task.

#### **4.4.3.1 Immediate Adjustment of a Sure Amount: MSoP Values and Spill-Over Effects**

Spill-over effects in the BREUT valuation model follow the same logic as the RP model, described in section 3.2. As explained in section 4.4.1, if average BREUT MSoP values were consistent with BREUT choices, they would result in valuations that are equal to the sure amount at the SI point (note that *valuation* = *sure amount* + *MSoP*). Comparing the difference between valuations and the SI point dependent on the previous choice shows how a spill-over of mental evidence influences MSoP mismatches. Simulated choices are between the lottery and the sure amount at the SI point. In case of lottery choices, the lottery CE sample incorporated into the MSoP valuation process is above the SI point. And in case of sure amount choices, it is below the SI point.

Observing MSoP mismatches at the SI point allows us to separate between the effects of these two types of mental evidence spill-overs. In the lottery choice case, the CE sample is above the SI point and the valuation will be biased upwards. In the opposite case of a sure amount choice, it will be biased downwards.

To observe the difference, we need to identify MSoP mismatches in the valuations contingent on choices between each lottery and their SI point's sure amount at £9.00. The average difference between the valuation and the SI point will be the remaining mismatch.

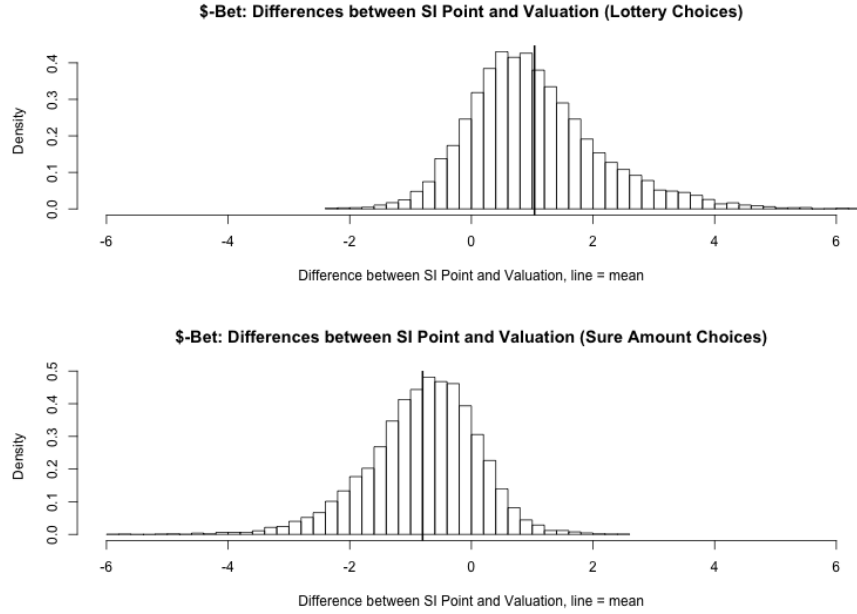
Figures 4.9 and 4.10 show histograms of P-Bet and \$-Bet MSoP mismatches after choices at the SI point. There is a positive MSoP mismatch for P-Bet and \$-Bet valuations that follow choices at the SI point. If valuations were equal to the amount that the DM chooses over the lottery 50% of times, their difference to the SI point would be zero on average. Instead, the average valuation is too high, it “overshoots” the SI point for the P-Bet (by £0.25) and the \$-Bet (by £0.98).

The same figures also show valuations that overshoot the SI point when they follow sure amount choices. But now they systematically overshoot the SI point in the other direction, resulting in valuations below the SI point both for the P-Bet (by £0.36) and the \$-Bet (by £0.85).

Figure 4.9: BREUT: MSoP Mismatches of Valuations following Choices at the P-Bet's SI Point



Figure 4.10: BREUT: MSoP Mismatches of Valuations following Choices at the \$-Bet's SI Point



### Deliberation Time and Immediate MSoP Values

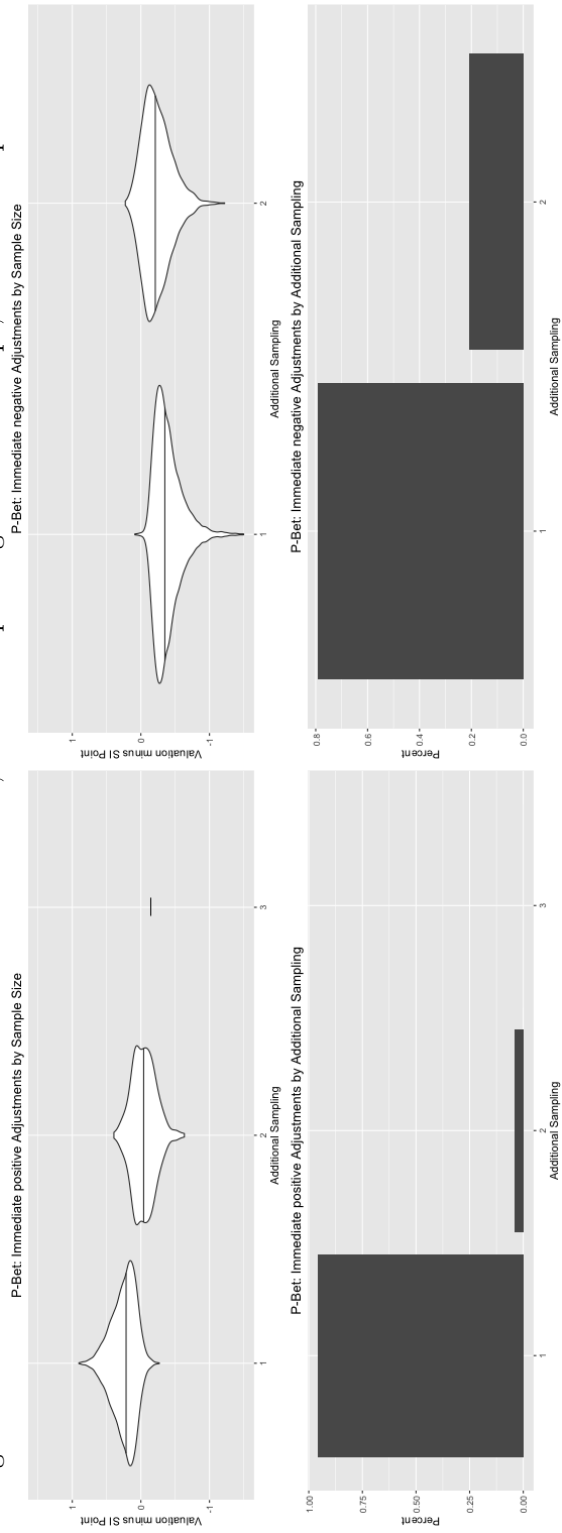
An MSoP valuation is treated by the model similarly to a regular valuation, only with a CE sample that already exists. Since the DM samples from an unbiased CE distribution, the valuation model predicts that the size of the MSoP mismatch decreases in deliberation time. Even if it is initially skewed, any increasing mental CE sample eventually approaches the mean of the underlying CE distribution. So the valuation model starts off with a CE sample with a positive MSoP mismatch and this mismatch decreases in deliberation time.

Figures 4.11 and 4.12 show violin plots of MSoP mismatches, separated by lottery type, choice type and sample size. This illustrates the marginal effects of longer deliberation times on the MSoP mismatch. For larger sample sizes, the valuation regresses to the mean and the respective MSoP value decreases. For both lotteries, DMs sometimes stop the process right at the start because the valuation sample's coefficient of variation is already sufficiently low. But this rate is much

higher for the P-Bet (at 85%) than for the \$-Bet (at 3%).

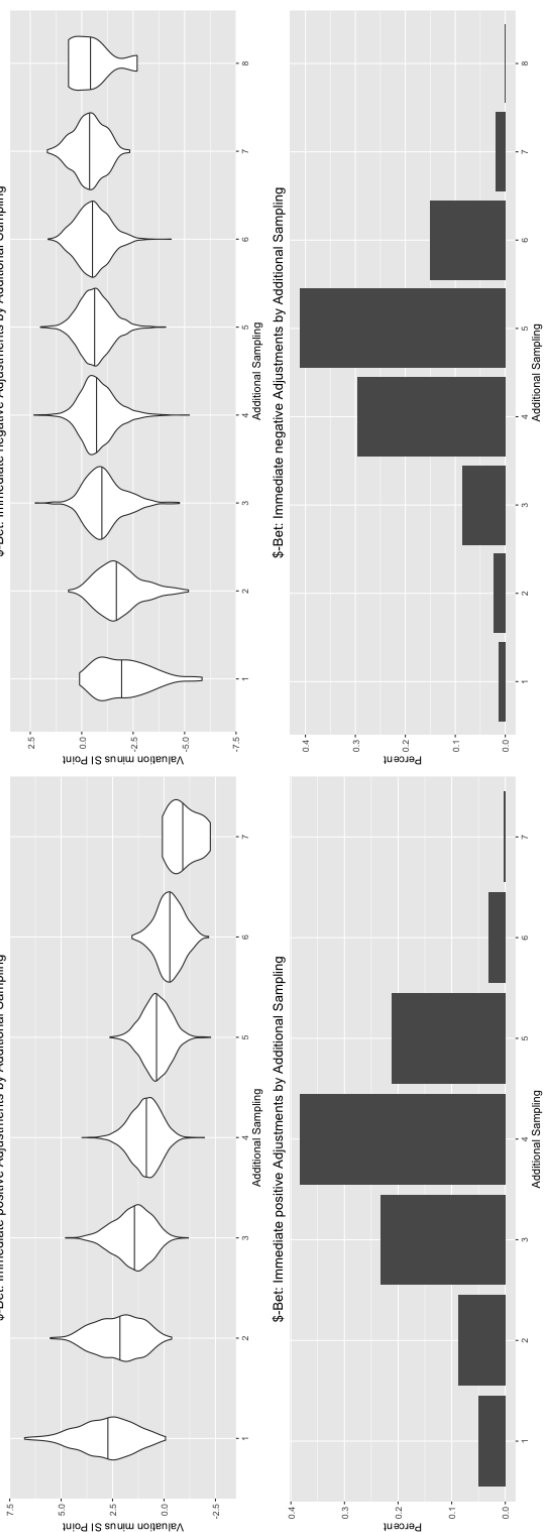


Figure 4.11: BREUT P-Bet: Immediate MSoP Mismatches, their corresponding Number of Steps, and their frequencies



Note: Median values are marked by horizontal lines

Figure 4.12: BREUT \$-Bet: Immediate Positive MSoP Mismatches, their corresponding Number of Steps, and their frequencies



Note: Median values are marked by horizontal lines

#### 4.4.3.2 Later Adjustment of a Sure Amount: MSoP Values and Consistency-Seeking Behaviour

MSoP valuations that depend on a choice *display*, only display information that the choice occurred in the past. So the DM is not aware of any mental evidence that led to the choice. But despite starting this immediate adjustment task without prior mental evidence, the sampling process is now subject to consistency-seeking behaviour. Any mental evidence that is inconsistent with the displayed choice will be ignored. So for valuations subject to consistency-seeking behaviour, we assume that DMs start their valuation process without any prior mental evidence but with a restricted distribution of CE values to sample from.

I.e., knowing that she chose the lottery over a sure amount at the SI point, the DM only considers lottery CEs *above* the SI point. Vice versa, all potential lottery CEs will be *below* the SI point if she chose the sure amount at the SI point over the lottery. Similarly to spill-over effects, this leads the valuations to overshoot the SI point,. This results in a positive MSoP mismatch both for positive as well as negative adjustments. But since no prior CE is incorporated, the BREUT valuation process does not start off with a biased CE sample. Instead, the BREUT valuation process values the lottery within the restricted CE distribution.

Figures 4.13 and 4.14 show violin plots of MSoP mismatches, separated by lottery type, choice type, and sample size. Because of the restriction of potential CE values to a plausible range, *all* resulting valuations overshoot the SI point and result in a positive MSoP mismatch. Compared to spill-over effects, this predicts a larger average MSoP mismatch both contingent on lottery choices (P-Bet: £0.45; \$-Bet: £2.85) and on sure amount choices (P-Bet £0.60; \$-Bet: £2.74).

Figure 4.13: BREUT: MSoP Mismatches of Valuations with Cognitive Dissonance due to Choices at the P-Bet's SI Point

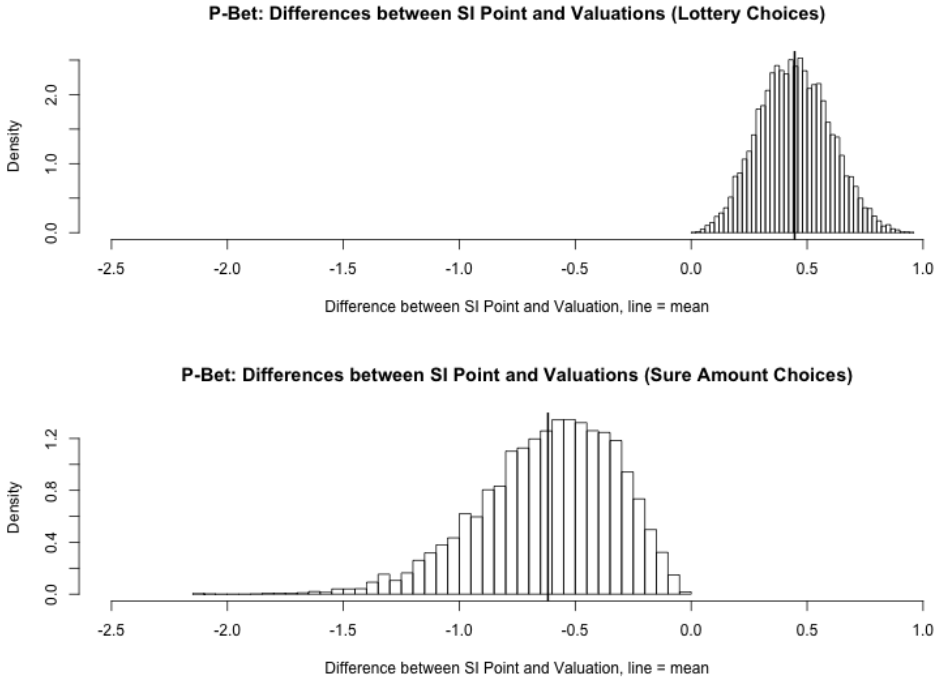
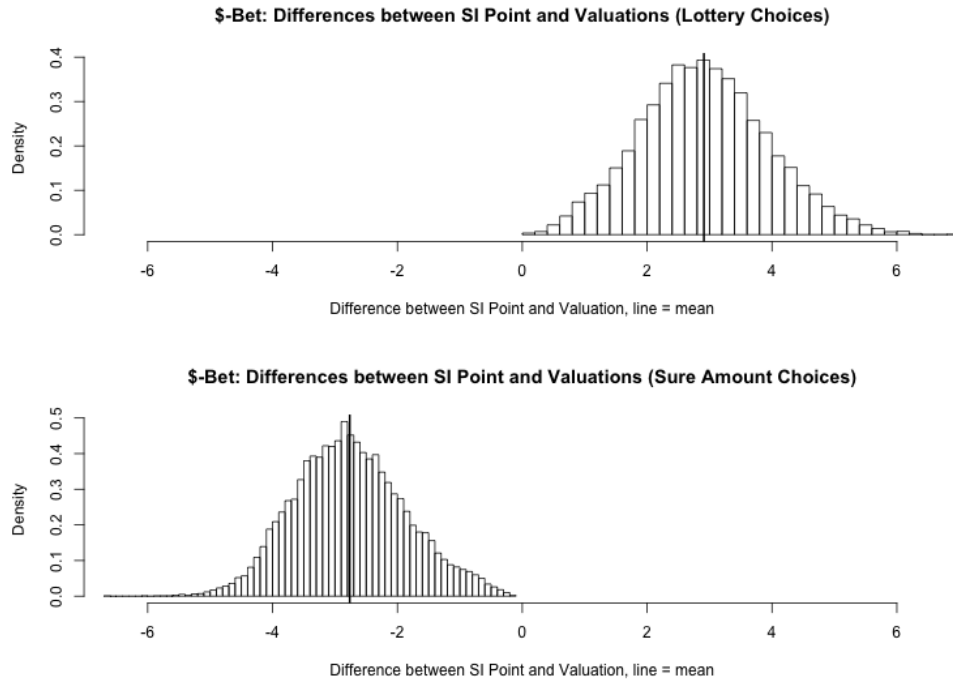


Figure 4.14: BREUT: MSoP Mismatches of Valuations with Cognitive Dissonance due to Choices at the \$-Bet's SI Point



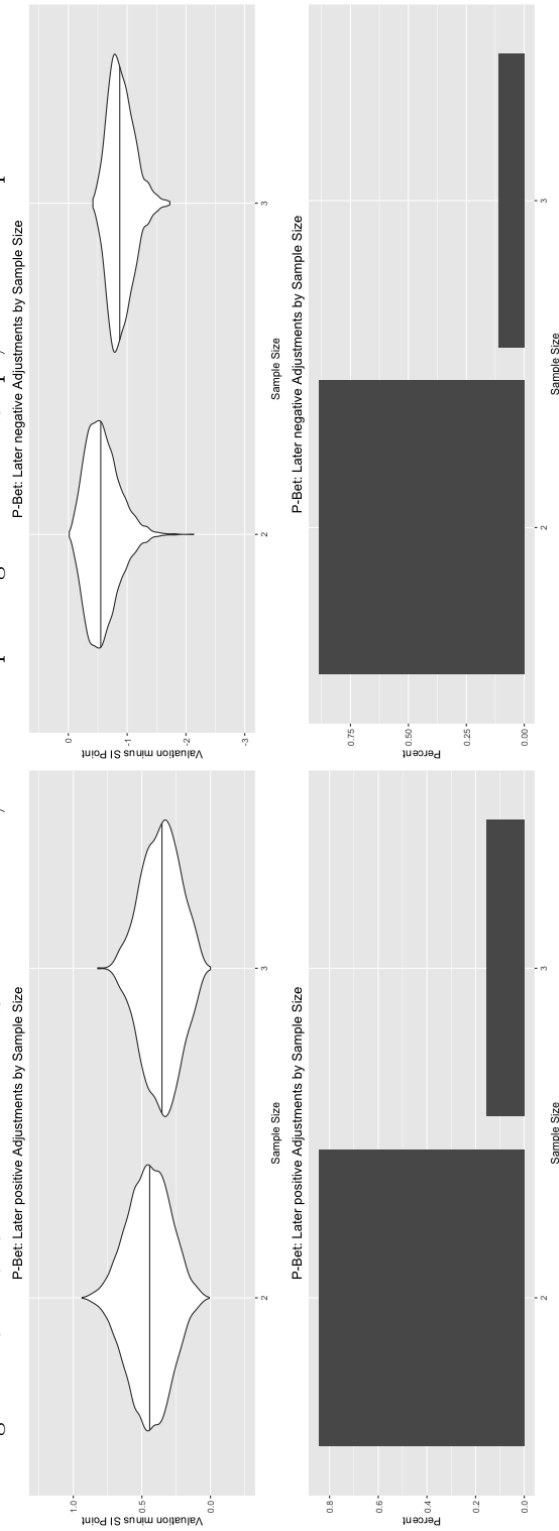
### Deliberation Time and Later MSoP Values

For consistency-seeking behaviour, BREUT follows the same dynamics over deliberation times as in the direct valuation case, only that the underlying CE distribution is truncated. Similar to a valuation process, the DM stops the process earlier for higher valuations and stops later for lower valuations. And as the DM has no access to any previous CE samples, she needs to start off the valuation process with sampling 2 CE values in order to be able to use the stopping rule.

Figures 4.15 and 4.16 show histograms of P-Bet and \$-Bet MSoP mismatches dependent on displays of choices between the lottery and the sure amount at the SI point. In all cases, valuations decrease in sample size. But this has a partly different effect on the mismatch than in the case of spill-over effects. For positive adjustments following lottery choices, the MSoP mismatch decreases in sample size (similar to spill-over effects).

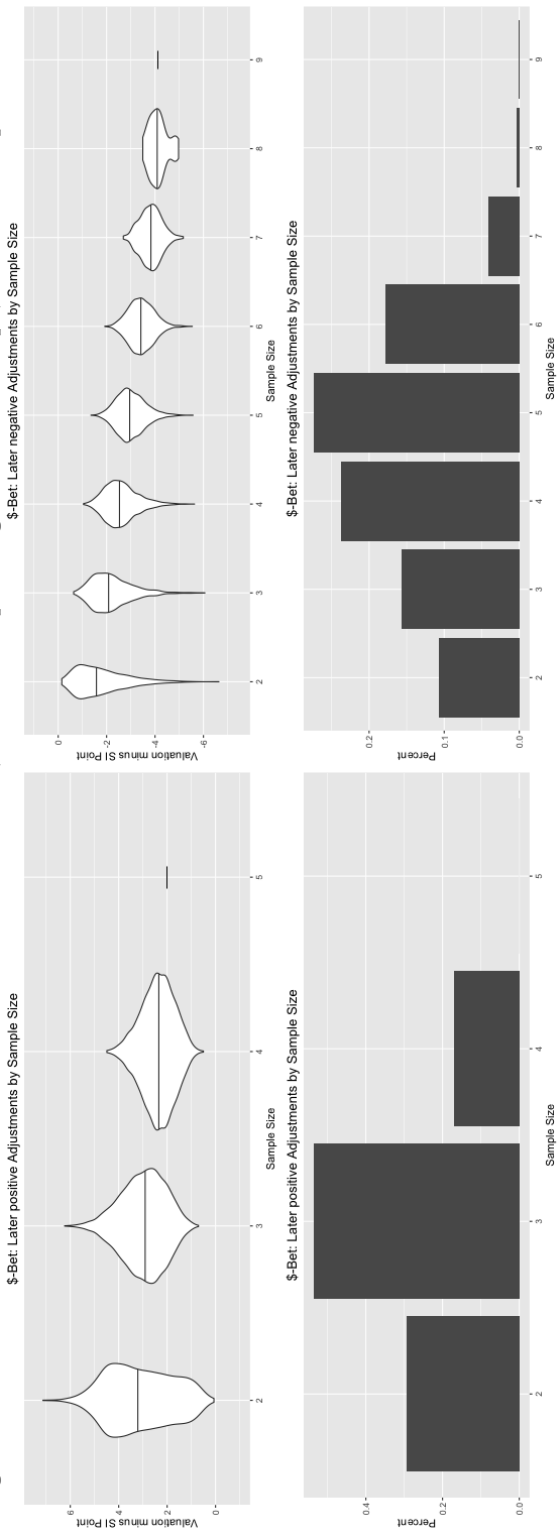
But for negative adjustments following sure amount choices, the MSoP mismatch increases in sample size. This is because a valuation process subject to consistency-seeking behaviour is equivalent to a direct valuation, only that it occurs within a restricted range. Section 4.3.3.3 already showed that larger samples result in lower valuations. Since this valuation process only samples from the minimum CE up to the CE at the SI point, any decrease in valuation will move the average valuation further from the SI point. Thereby, a greater sample size results in average MSoPs that become bigger as they are based on valuations further below the SI point.

Figure 4.15: BREUT P-Bet: Later MSoP Mismatches, their corresponding Number of Steps, and their frequencies



Note: Median values are marked by horizontal lines

Figure 4.16: BREUT \$-Bet: Later Positive MSoP Mismatches, their corresponding Number of Steps, and their frequencies



Note: Median values are marked by horizontal lines



## 4.5 Comparison to existing Boundedly Rational Valuation Models

### 4.5.1 Similarities

#### Choices

All models use a core model that predicts probabilistic choices. While the models use different methods for these predictions, they all use parameters where the P-Bet is stochastically preferred over the \$-Bet in choices to predict a preference reversal. All core models are stochastically transitive while explaining a problem of stochastic intransitivity.

#### Valuations

In all models, the processes that use the core models are different for choices and valuations. This results in preferences that can be intransitive *across* these two elicitation methods. The SP model and the SVM mechanism achieve this by assuming that valuations are made through an iterative choice process while the BREUT valuation model assumes that valuations are made through sequential sampling of CE values.

All boundedly rational valuation models discussed in this and the previous chapter feature a valuation process that converges towards an underlying value until the stopping rule ends the process. This underlying value also represents a preference in choices, and is equal or lower for the \$-Bet than for the P-Bet (given the parameters that predict a preference reversal). But for each model, the valuation process is built in a way that initial valuations correspond to a preference reversal early in the process. Early in the process, \$-Bet valuations are often higher than P-Bet valuations. Only if the stopping rule triggers these candidate values to be reported early enough in the process, will the DM report valuations that are still *higher* for the \$-Bet than for the P-Bet. But the longer sampling goes on during the process, the higher the probability that P-Bet valuations become higher than

\$-Bet valuations. Then, a DM will not report valuations that constitute a preference reversal.

#### 4.5.2 Differences

##### Valuations

Unlike the existing models, the novel valuation model uses a “full sampling” process to predict valuations. Instead of adjusting a single CE sample, the model’s DM continually samples from an underlying distribution of mental evidence.

Also, the novel model predicts that both types of lotteries are overvalued when compared to their SI points. The preference reversal is predicted because the size of the overvaluation is larger for the \$-Bet than for the P-Bet.

In addition, the sampling approach does not predict that the size of the adjustment influences the length of the sampling period. Candidate values are assumed to be continually updated to equal the mean of the CE sample and not to be changed in steps along the lottery’s range. Therefore, valuing a lottery that was chosen over £0 for sure does not take longer than valuing a lottery that was chosen over the sure amount at the SI point. The existing models predict that the size of the final MSoP corresponds to the duration of the valuation process.

##### MSoP Effects: Spill-Over Effects and Consistency-Seeking Behaviour

The novel model also allows the sampling process to be contingent on spill-over effects and consistency-seeking behaviour (as described in section 4.4). A preference can spill over from a choice to a subsequent valuation and affect individual valuations, predicting a positive MSoP mismatch. Cognitive dissonance can restrict the mental evidence that the DM uses for the sampling process, causing consistency-seeking behaviour. This also results in a positive mismatch. These MSoP mismatches are positive both for upward and downward adjustments.

The existing models do not predict a positive MSoP mismatch but a negative one for both adjustment types. Furthermore, they predict that the starting value of a valuation process entirely controls the preference reversal. \$-Bets are valued higher

than P-Bets because the starting value of their valuation process is higher. The new model’s valuation process does not depend on any initially considered candidate value but instead on prior choices of the DM.

### **4.5.3 Novel Aspects of the BREUT Valuation Model**

The BREUT valuation model’s calculation logic works in a novel way that differs from the existing models and makes several different predictions. But as the model assumes underlying parameters of individual preferences that cannot be measured directly, these predictions are limited to be qualitative, i.e. they only predict the direction and not the size of mismatches between choices and valuations.

Still, the predictions demonstrate that an underlying preference distribution generated by classic “rational” EUT functions can produce several interesting predictions if assumed to be subject to a boundedly rational decision process.

The model’s novel aspects can be summarised in four categories:

#### **1) Valuation Process**

The novel model assumes a “full sampling” valuation process, where the valuation entirely depends on a CE sample. I.e., a valuation sample is endogenously generated from the same underlying distribution of CEs that is used for choices.

Despite using an identical CE distribution across procedures, the model predicts an overvaluation effect compared to the lottery’s SI point. This effect is stronger for the \$-Bet, which can lead to systematic preference reversals: \$-Bets can be placed higher than P-Bets in average valuations despite being preferred less in choices.

#### **2) Stopping Rule**

Existing models already use endogenous stopping rules for valuation processes. A choice between the valuation and the lottery at each step determines if the valuation appears to be distinguishable in its subjective utility from the lottery.

The BREUT valuation model also uses an endogenous stopping rule but it incorporates information from the entire sample that is used for a valuation. This stopping rule also allows a longer sampling phase for a lottery that has a larger variance in underlying CEs.

### **3) Spill-Over of Mental Evidence**

The model demonstrates a mechanism through which mental evidence on the strength of preference in a choice can be carried over into a subsequent MSoP valuation process. The same CE sample that determined the choice is used in building a subsequent valuation, quantifying how much one option is preferred over another through an MSoP valuation.

### **4) Restriction of Mental Evidence**

The model can incorporate cognitive dissonance effects into the valuation process by restricting the distribution of underlying CE values. As a result, the DM reports MSoP valuations that show a bias towards being consistent with a specific prior choice instead of the underlying mean of all prior choices.

#### **4.5.4 Testable Differences to Existing Models**

The BREUT valuation model makes several predictions that have a different direction than those of existing models. This section details the three main predictions of the BREUT valuation model that are novel and unique. In short, the model predicts average lottery valuations above the respective SI points, and a positive MSoP mismatch but only predicts a weak effect size for the preference reversal phenomenon. Therefore, before assessing the exact model fit, it is more sensible to test which models are better at predicting behaviour at a qualitative level. This then gives an intuition in what ways the model contributes to improving theoretical foundations for predicting choice-matching discrepancies.

The differences in qualitative predictions are summarised in table 4.5 along with the RP model as a benchmark for conventional economic theory. If these

predictions are confirmed in a controlled experiment, then the BREUT valuation model can be judged to be more accurate in predicting effect directions in the tasks described throughout this chapter. If not, it is sensible to check in what respect existing models perform better to decide if any of the models can adequately describe behaviour in these tasks.

Table 4.5: Qualitative Predictions of the three described Models

		Average Model Predictions				
	Lottery	RP Model	SVM Mech.	SP Model	BREUT Valuation	
Phenomenon	P-Bet	No	Yes	Yes	Yes	
		= 0	< 0	< 0	> 0	
		> 0	< 0	< 0	> 0	
		MSoP-Mismatch				
		MSoP-Mismatch after choice display	= 0	< 0	< 0	> 0
	Longer RTs* effect on immediate MSoP Mismatch	= 0	< 0	< 0	< 0	
	\$-Bet	Preference Reversal	No	Yes	Yes	Yes
		Valuation - SI Point	= 0	> 0	> 0	> 0
		MSoP-Mismatch	> 0	< 0	< 0	> 0
		Mismatch after choice display	= 0	< 0	< 0	> 0
Longer RTs* effect on immediate MSoP Mismatch		= 0	< 0	< 0	< 0	

\*RTs := Reaction Times

#### 4.5.4.1 Preference Reversals and SI Points

All models were developed to predict the classic preference reversal, so its prediction does not serve as a distinguishing measure. But they differ in how they predict it in relation to SI points. BREUT valuations of all lotteries are biased upwards relative to their SI points. But this bias is not the same size for all lotteries. Valuations of riskier lotteries are overvalued more than those of safer lotteries. This bias can become so large that on average the following applies  $SI(\$) < SI(P) < valuation(P) < valuation(\$)$ <sup>7</sup>. This constitutes a preference reversal because the P-Bet is preferred in choices while the \$-Bet is preferred in valuations.

The SP model and SVM mechanism predict that the preference reversal is generated by undervaluing the P-Bet while overvaluing the \$-Bet in relation to their SI points. These models therefore predict that a median P-Bet valuation will be below the P-Bet's SI point and a median \$-Bet valuation will be above the \$-Bet's SI point. But the BREUT valuation model predicts that *both* lotteries' median valuations will be above their respective SI points.

The experiment reported in chapter 5 tests for this by estimating participants' SI points as well as eliciting their valuations of the P-Bet and the \$-Bet. As all models do predict the preference reversal, the criterion to determine the superior model is to estimate the relation between SI points and median valuations at the individual level. If lotteries are consistently valued higher than their SI points, existing models are rejected in favour of the BREUT valuation model.

#### 4.5.4.2 MSoP Mismatch

Considering the MSoP Mismatch, it is even simpler to distinguish between predictions of the "Choose and Adjust" models and the BREUT valuation model. These predictions will cover the simple case of choice and MSoP tasks between a lottery (P-Bet or \$-Bet) and a sure amount. After a choice between the two options, the

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<sup>7</sup>Note that BREUT choices are stochastically transitive. So on average  $SI(\$) < SI(P) \iff \$ \prec P$ .

DM adjusts the sure amount through a valuation process until it is equally as attractive as the lottery. The mean difference between the self-consistent MSoP (the adjustment necessary to make the sure amount equal to the lottery’s SI point) and the actual MSoP will be defined as an MSoP mismatch.

“Choose and Adjust” models predict that an average MSoP mismatch is always negative. In contrast, the BREUT valuation model predicts that the MSoP mismatch is always positive (see subsections 4.4.3.1 and 4.4.3.2). But this mismatch can be positive for two different reasons.

First, a spill-over effect from an immediately previous choice between a lottery and a sure amount can influence a subsequent valuation of a lottery in that choice (comparable to the MSoP setting designed by Butler et al., 2014). In this case, the mental evidence supporting the choice is also used in the subsequent valuation. This evidence must have supported the choice, otherwise the MSoP task would apply into the other adjustment direction. Therefore, spill-over evidence will always have a positive effect on the mental sample for an MSoP task.

Second, information about previously *having made* such a choice can also influence a subsequent BREUT valuation process (“Choose and Adjust” models do not apply to this scenario). Consistency-seeking behaviour can restrict the generation of mental evidence. In that case, mental evidence that is inconsistent with the previous choice is ignored in the sampling process. Therefore, consistency-seeking behaviour has a positive effect on MSoP values as “unsupportive” evidence is ignored. This feature is a potential “add-on” to the model.

The experiment in chapter 5 can determine if this add-on improves predictions. An experimental setting can distinguish between the two MSoP cases by altering the delay between choice and subsequent MSoP. If an MSoP task follows right after the respective choice, mental evidence can still be present in a participant’s working memory and both spill-over effects as well as consistency-seeking behaviour are possible. If not, only consistency-seeking behaviour is possible. Since the effects add on to each other, the BREUT valuation model predicts that MSoP values are positively biased, and more so in case of MSoP tasks right after the choice.



The “Choose and Adjust” models therefore predict that an MSoP mismatch is always negative while the BREUT valuation model predicts that an MSoP mismatch is always positive. Therefore, the criterion to determine the superior model is to estimate the average MSoP mismatch. If it is consistently positive, existing models are rejected in favour of the BREUT valuation model.

There is also an additional opportunity to test the BREUT valuation model prediction that spill-over effects increase the MSoP mismatch when an MSoP task follows right after a choice. To determine if the BREUT valuation component of consistency-seeking behaviour improves predictions, the difference in mismatches between MSoP tasks right after the choice and MSoP tasks delayed after the choice need to be estimated. If MSoP mismatches are always positive but higher right after the choice, incorporating consistency-seeking behaviour into the BREUT valuation model does lead to better predictions.

#### **4.5.4.3 Overvaluation and Marginal Effects of Reaction Times**

Few experiments investigated differences between valuations and SI points and it has not been tested if this is subject to marginal effects of longer deliberation times. This is especially interesting because all simulated specifications of boundedly rational valuation models predict that longer deliberation times reduce the discrepancy between valuation and SI point.

While “Choose and Adjust” models predict that valuations start off too low for P-Bets and too high for \$-Bets, the BREUT valuation model predicts that average valuations start off too high for both lotteries. Everything else equal, average P-Bet valuations in “Choose and Adjust” models then increase with deliberation time while \$-Bet valuations decrease. The BREUT valuation model instead predicts that lottery valuations of both P-Bets and \$-Bets decrease in longer deliberation times.

But all models do predict that the discrepancy between valuations and SI points decreases for longer deliberation times. Not only does the extent of over- and undervaluation decrease, the likelihood of a P-Bet valuation below a \$-Bet valuation

also decreases for longer deliberation times.

Chapter 6 deals with this, using additional reaction time data from the experiment in chapter 5. Even though deliberation times are not directly observable in experiments, reaction times are (see the section 2.5.1 on reaction times in chapter 2 for a distinction). A necessary assumption to apply this to reaction times in experiments is that longer deliberation times do lead to longer reaction times in experiments (when controlling for individual- and task-specific effects). Chapter 6 investigates such reaction time effects for direct valuations.

## 4.6 Contribution and Discussion

Existing boundedly rational valuation models can predict the classical preference reversal through an iterative adjustment process of a single mental sample of a valuation (Johnson and Busemeyer, 2005; Blavatsky and Köhler, 2009b). But this process rules out the possibility of mental evidence being influenced by spill-over effects from a previous evidence-gathering phase or consistency-seeking behaviour. These models also do not predict the tendency of participants in experiments to overreport their strength of preference in monetary units (MSoP) as described by Butler et al. (2014a).

This chapter presented a model of valuation that uses a novel sampling approach to address these weaknesses. The model assumes that DMs sequentially sample from an underlying distribution of CE values for a valuation. As they stop accumulating CE samples as soon as the coefficient of variance falls below a threshold of confidence for them, they tend to stop the process earlier for higher valuation samples. This results in final valuations that are higher than the mean of the underlying CE distribution. DMs use the same CE distribution in a sequential sampling process for choices, as described by the BREUT model (Navarro-Martinez et al., 2017). Despite using transitive EUT functions for the generation of CE samples, they overvalue high-variance lotteries more than low-variance lotteries compared to the lotteries SI points. Thereby, the model predicts a preference reversal even though

DMs rely on the same underlying distribution of EUT functions to generate mental evidence for both lottery types. So despite stochastic transitivity in choices and the same underlying “rational” distribution of mental evidence, the model predicts violations of procedure invariance.

In addition, the model can incorporate a spill-over of mental evidence from a choice to a subsequent valuation. This spill-over effect leads to a positive MSoP mismatch. In this case, longer reaction times are predicted to coincide with decreasing MSoP mismatches.

Through an extension, the model can also predict cognitive dissonance effects in the form of consistency-seeking behaviour, where a DM disregards mental evidence that is dissonant with their previous choice. This consistency-seeking behaviour also leads to a positive MSoP mismatch.

Compared to the model developed in chapter 4, existing boundedly rational valuation models make several fundamentally different predictions. Existing models do not predict an overvaluation of all lottery types compared to their SI points, instead they predict overvaluations of high-variance lotteries and undervaluations of low-variance lotteries. Existing models also predict negative MSoP mismatches for positive and negative adjustments of a sure amount after a choice between a lottery and a sure amount. This provides the case for a controlled experiment to test which model’s predictions adequately capture behaviour in choice and valuation at the participant level.

## Chapter 5

# How Choice Preference relates to different Methods of Valuation: An Experiment

### 5.1 Introduction

A central prerequisite of classic economic theory is the estimation of individual preferences towards risk that determine behaviour. However, elicited preferences are only meaningful if they do not consistently contradict each other and thereby result in a reversal of preferences. Classic economic theory assumes procedure invariance, i.e. on average a participant's preferences do not change dependent on context and/or task type. Preference reversals and the MSoP mismatch are examples where this is not the case. This chapter reports an experiment that tests qualitative predictions from the models described in chapters 3 and 4. This is done by observing binary choices, direct valuations, and MSoP valuations.

It is also unclear whether the MSoP mismatch originates from the choice or the valuation aspect of the MSoP task. To test this, the experiment contrasts choices, valuations, and mixes of both. First, choices, valuations, and their mixes are constructed in a way that makes the *elicited* preferences comparable between

tasks. Second, an MSoP task, a “mix” of choice and valuation, is contrasted with a different version where participants need to estimate their preference given information for one of their *past* choices. This eliminates the “choice part” of the task while preserving the contingency of the valuation on a choice. What remains is the valuation task as well as any effect that the displayed information might have on the participant’s behaviour. This potential effect is assumed to be cognitive dissonance, which results in consistency-seeking behaviour.

In addition, choices are designed in a way to test how a valuation might be influenced by a particular sure amount on which it is contingent. E.g., an MSoP task involving a lottery and a sure amount of £0 implies a lower bound that is smaller than in an MSoP task between the same lottery and a sure amount of £5 as it leaves more “room” for different valuations. Vice versa for a sure amount that is equal to the maximum payoff versus a sure amount lower than this maximum.

The experiment’s results replicate the preference reversal as well as a positive mismatch in MSoP values for positive adjustments. However, participants do not show a significant difference in valuations between the MSoP task and its degenerate version without a previous choice. This suggests that the MSoP mismatch stems from consistency-seeking behaviour. However, the MSoP mismatch is negative for negative adjustments. Altogether, these results are incompatible with existing theory.

The chapter is organised as follows: After an explanation of the theoretical background, a description of the experimental design follows in Section 5.2. Section 5.3 summarises the hypotheses that are tested, section 5.4 presents experimental results, and Section 5.5 concludes with a discussion.

### 5.1.1 Theoretical Background

In order to understand the experimental procedure, it is helpful to first understand the baseline case where all preference elicitations are consistent with each other. See figure 5.1 for an illustration of P-Bet and \$-Bet CE elicitations in three different procedures: A) By inferring an SI point, B) through direct valuation, C) through

an MSoP Task.

A) Shows a typical observation of choice probabilities between P-Bet/\$-Bet lotteries against different sure amounts. Following Mosteller and Nogee (1951), the lottery choice probability decreases in choices against higher sure amounts. While the lottery is always chosen over £0, at the SI point the lottery choice probability is 50% for some sure amount.

B) Displays a consistent difference in CE values: the P-Bet is valued higher than the \$-Bet. Valuations are possible along the lotteries' ranges, with the \$-Bet's range being larger (not to scale in the figure). In addition, the difference in CEs quantifies the difference in attractiveness to the participant. The SI points from A) suggest CE differences in choices that reflect a preference for the P-Bet and this difference is also generated in the valuation procedure B).

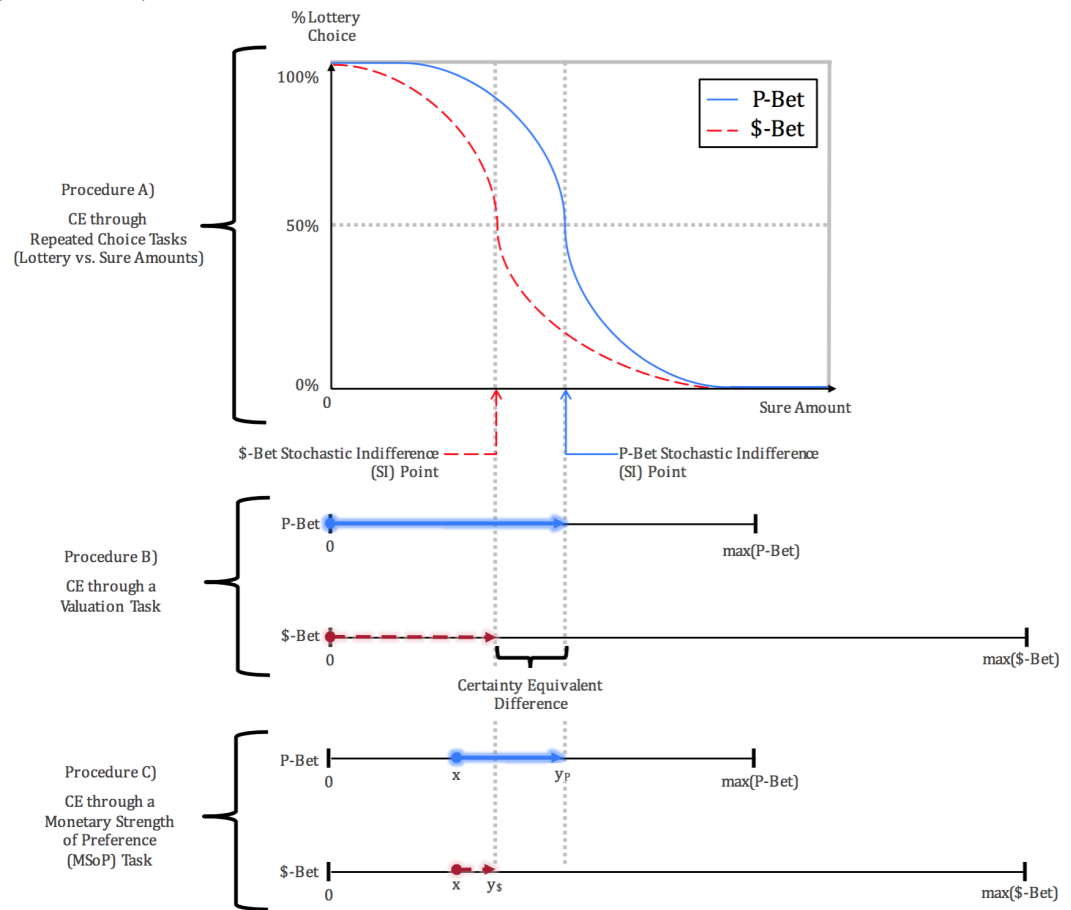
C) Shows a hypothetical response of a DM who had chosen the lottery over a sure amount  $x$  and was prompted for an MSoP. If her preferences would be consistent across procedures, she would value the lottery in the MSoP task to equal the sure amount at the SI point. Then, her MSoP added to  $x$  would equal the SI point's sure amount, with  $x + MSoP = SI$ . This CE difference is a strength of preference in monetary terms, the MSoP value.

In the figure, she states MSoP values consistent with procedure A) for both lotteries. E.g., for the P-Bet her valuation reaches the SI point with  $x_P + MSoP = y_P = SI_P$ . If  $y_P$  were consistently larger than her  $SI_P$ , the DM's MSoP values would show a positive mismatch for the P-Bet. MSoP values are also consistent with procedure B) as the P-Bet is again valued higher than the \$-Bet.

Participants that *only* state their MSoP between two lotteries without stating the respective CEs, still respond to something logically similar to a valuation task. Instead of reporting two valuations of two options, they only report a single valuation difference between the options. Therefore, any MSoP task is also a preference elicitation procedure. Figure 5.1 constructs an example where different procedures generate the same CE values.

But the preference reversal as well as the MSoP mismatch show that this is not always the case. Still, it is not clear where these inconsistencies stem from. The question remains if eliciting a CE value in one procedure might affect the elicited CE value in another procedure. E.g., if a choice task (procedure A) is immediately followed by an MSoP task (procedure C). While the MSoP mismatch might stem from the valuation procedure alone, it is not possible to rule out its connection to a previous choice as a causal factor.

Figure 5.1: Three different methods to elicit P-Bet/\$-Bet Certainty Equivalents (not to scale)



#### 5.1.1.1 Preference Reversals

Chapter 4 already gives the qualitative prediction of an inconsistency between choices (procedure A) and valuations (procedure B). The BREUT valuation model predicts that a majority of valuations is higher than the SI point of a lottery.

#### 5.1.1.2 Spill-Over Effects

The BREUT valuation model predicts that incorporating mental evidence from a choice phase into an MSoP valuation leads to a positive mismatch in the stated MSoP. Even if the MSoP valuation is not entirely based on the pre-existing mental evidence base, it is possible that it does cause a mismatch in an otherwise unbiased valuation. In the example of procedure C) in figure 5.1, this means that the DM will on average state a too high MSoP value that overshoots the SI point ( $x_P + MSoP^{Mismatch} = y_P^{Mismatch} > SI_P$ ).

Furthermore, this positive mismatch in MSoP values will go into both directions. If a participant finds a lottery less attractive than on average, a valuation based on this mental evidence base will be biased towards a lower than average value. This will be reflected in the MSoP, which now quantifies a downward adjustment of the sure amount. So if the DM now adjusts downwards too much, the MSoP value also shows a positive mismatch. In both cases, due to the spill-over of mental evidence from choice to MSoP process, adjustments result in too high MSoP values and overshoot the SI point. Butler et al.'s (2014a) experiment was only designed to test for positive MSoP because it only allowed positive adjustments of a rejected option. Therefore, it remains unclear if MSoPs overshooting the SI point result from the valuation process, which overvalues lotteries' valuations compared to their SI points, or from spill-overs of mental evidence.

We assume that this spill-over effect can only exist in MSoP tasks that occur straight after a choice. So as soon as DMs are not aware of the choice process anymore, they will not be able to access their mental evidence anymore. It follows that the spill-over effect will disappear as soon as the MSoP task is sufficiently delayed in time from the respective choice. However, participants might still be



influenced; not by a spill-over of mental evidence but just by being aware that they made a certain choice in the past.

### 5.1.1.3 Consistency-Seeking Behaviour

BREUT can also incorporate consistency-seeking behaviour into its predictions, also predicting a positive MSoP mismatch. Unlike with spill-overs of mental evidence, we cannot assume that an MSoP mismatch resulting from consistency-seeking behaviour disappears as soon as a participant is not aware of their choice anymore. If a participant has forgotten their choice but then sees the display of an MSoP task, she will be reminded of her past choice. The BREUT valuation model with consistency-seeking behaviour predicts that she will take this choice at face value and then base her reasoning on the fact that her preference supported that choice. Therefore, consistency-seeking behaviour cannot be removed from MSoP tasks.

But it would be possible to compare MSoP tasks immediately after choices to delayed MSoP tasks that are contingent on the same choices in the past. Thereby, we can distinguish if the MSoP mismatch originates from the choice or the valuation aspect of the MSoP task. An MSoP task using an immediate adjustment can be compared with a version where the participant needs to estimate her preference given one of her *past* choices in an MSoP task using a delayed adjustment. This rules out an influence of the choice process on the MSoP valuation but keeps the influence of the choice display. So there is only a valuation process influencing the MSoP task along with the potential effect of the choice display. This effect is assumed to be consistency-seeking behaviour. Section 5.3.3 explains how hypotheses distinguish between consistency-seeking behaviour and spill-over effects.

## 5.2 Experimental Design

The experiment used 4 different types of tasks that all generated information on a participant's CE, albeit through different procedures. In Task 1: the *SI* Task, CE values were generated by presenting repeated tasks between a lottery and various

sure amounts and inferring which theoretical sure amount would be preferred by the participant in 50% of the repetitions. This provides individual-specific SI points. In Task 2: the *IA* Task, participants were prompted right after a *SI* Task choice to adjust the offered sure amount with a slider until they found it to be equally as attractive as the lottery, resulting in a CE. Task 3: the *LA* Task, instead featured the *display* of one of the participant’s past choices between the lottery and a sure amount, also prompting them to adjust the sure amount towards a CE that they found to be equally as attractive as the lottery. In Task 4: the *DV* Task, participants were presented with a standard valuation task where they had to select their CE for the lottery on a slider.

The experiment was conducted using computers in the Behavioural Science Laboratory at Warwick Business School, UK. 77 students from the SONA subject pool of the University of Warwick completed the experiment. Three participants showed non-compliance with the incentivisation procedure by stating a valuation close to the possible maximum in almost all cases and their data was excluded (see Appendix Section A.2 for exclusion criteria). This leaves data from 74 participants for the analysis.

Including distractor tasks and attention checks, the experiment featured 300 tasks for each participant, divided into two parts. Both parts included an introduction and were separated by a questionnaire comprised of the health&safety and social dimension of the revised domain-specific risk taking scale by Blais and Weber (2006). Task types were referred to in a colour-coded header to ensure that participants were able to distinguish between task types. The respective colour was randomly determined for each participant by the computer program that displayed the tasks. Part 1 featured 204 trials with task types *SI*, *DV*, and *IA* while Part 2 featured 96 trials with task types *SI*, *DV*, and *LA*.

The task order was randomised but restricted by several conditions: I) *SI* Task trials featuring the main lottery parameters (not distractors) were only displayed in Part 1; II) *IA* Task trials were only displayed in Part 1; III) *LA* Task trials were only displayed in Part 2, as they were dependent on *SI* Task trials and to avoid

confusion with *IA* Task; IV) To avoid awareness of past responses by participants, any main lottery needed to be followed by at least two distractor lotteries before it was featured again in a task.

Because of this, the trial order was not fully random but this served three purposes: First, participants would not complete tasks with identical lotteries straight after another, which might lead to interference in responses. Second, participants were less prone to misunderstanding if displayed options in *IA* and *LA* Tasks were from an immediately previous choice or a past choice. Third, *LA* Task choice displays were administered based on all the participant's data on past choices, thereby including all *SI* Task data from the main lotteries.

### 5.2.1 Task 1: $CE_{SI}$ through Choices

The choice part of the experiment involved binary choices between a lottery and a sure amount (see figure 5.2). Two monetary lotteries were used as P-Bet and \$-Bet<sup>1</sup>: a 80% chance of winning £12 and a 25% chance of winning £50, respectively. Participants chose between playing out the P-Bet and a number of different sure amounts with equal parts either only facing even  $\{0; 2; 4; \dots; 10\}$  or odd  $\{0; 3; 5; \dots; 11\}$  numbers (but both times including zero). Similarly, the \$-Bet was displayed along the same sure amounts.

A binary choice between the P-Bet and the \$-Bet was also added to allow an estimate of the participant's preference in direct choices between the two. All trials but the single choices against £0 were repeated 5 times, thereby generating data on 57 binary choices for each participant. From this data, an SI point is inferred at which each specific participant would show a 50% probability between choosing the lottery and the sure amount. This sure amount need not be featured in actual choices but might also lie between two sure amounts, e.g. £6.5 for the P-Bet. This is the CE elicited through the SI Point,  $CE_{SI}$ .

$CE_{SI}$  can be inferred by logistic regression or by counting the absolute number of lottery choices as a proxy for the SI point along the range of sure amounts.

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<sup>1</sup>The preference reversal phenomenon was already established for these parameters by Loomes and Pogrebnia (2016).

Figure 5.2: Straight Choice in the *SI* Task (Task 1)

Task No. 1							
<b>Choice Task</b>							
Please choose the option you would prefer to play out.							
• A	<table border="1" style="width: 100%; border-collapse: collapse; text-align: center;"> <tr> <td style="width: 50%; padding: 2px;">1 - 20</td> <td style="width: 50%; padding: 2px;">21 - 100</td> </tr> <tr> <td style="background-color: #add8e6; padding: 5px; font-size: 1.2em;">£0</td> <td style="background-color: #add8e6; padding: 5px; font-size: 1.2em;">£12</td> </tr> <tr> <td style="padding: 2px;">20%</td> <td style="padding: 2px;">80%</td> </tr> </table>	1 - 20	21 - 100	£0	£12	20%	80%
1 - 20	21 - 100						
£0	£12						
20%	80%						
• B	<table border="1" style="width: 100%; border-collapse: collapse; text-align: center;"> <tr> <td style="padding: 2px;">1 - 100</td> </tr> <tr> <td style="background-color: #add8e6; padding: 5px; font-size: 1.2em;">£6</td> </tr> <tr> <td style="padding: 2px;">100%</td> </tr> </table>	1 - 100	£6	100%			
1 - 100							
£6							
100%							
Once you are satisfied with your decision, please proceed to the next task.							
<input type="button" value="Next Task"/>							

Since a logistic regression needs to be fitted for each participant and is more susceptible to noise, the counting method was used, following Loomes and Pogrebná (2016):

If, e.g., a lottery was chosen over sure amounts  $\{2; 4; 6; 8; 10\}$  10 out of 25 times, the cut-off point will be inferred to be “ten steps” towards the highest sure amount at £10. The steps are determined by dividing the range from the minimum to the maximum sure amount by the total number of choices, in this case 25. Regardless of the precise sure amounts over which the lottery was preferred, these steps are added to the lowest amount, resulting in an inferred SI point of  $£2 + (£10 - £2) \cdot 10/25 = £5.20$ .

### 5.2.2 Task 2: $CE_{IA}$ through Adjustment Tasks following Choices

On some occasions, an adjustment task (*IA* Task, see figure 5.3) was added on to a choice task (*SI* Task). Participants were prompted right after their binary choice to adjust the sure amount so that they find it equally as attractive as the lottery. This adjustment provides another CE through an immediate adjustment of a sure amount  $CE_{IA}$ , that can be compared to  $CE_{SI}$ . In combination with the sure amount of the binary choice it also provides an MSoP value:  $|CE_{IA} - \text{Sure Amount}| = \text{MSoP}$ . In order to be consistent with their choice, a participant will need to decrease the sure amount after having chosen it or increase it if they had instead chosen the lottery.

Note that the adjustment is different to the design of Butler et al. (2014a), where participants needed to only state *positive* adjustments to all outcomes of the *rejected* option. This money amount was their MSoP that made both choices equally as attractive to them. Their design did not always allow for the calculation of a CE that logically followed from the task: In the case of a binary choice between two lotteries, both the preferred and the improved lottery would still involve risky payoffs. In addition, there is a risk that participants only focussed their attention on one of the payoffs when stating their MSoP. The design of this experiment avoids these issues because it generates a clearly displayed CE with each task that is also visible to participants as they give their responses.

In addition, participants had the option to state a CE that logically contradicts their past choice, thereby showing that they changed their preference. E.g., if a participant has chosen £6 over the P-Bet but has changed their preference, they can reconsider their choice and state a CE above £6 by adjusting the sure amount again. This logically contradicts their past choice since a CE of more than £6 implies that they do prefer the P-Bet over £6.

Adjustment tasks were randomly predetermined for two out of five repetitions. Therefore, 2 repetitions of the binary choice *SI* tasks between a specific lottery and a specific sure amount were followed by an *IA* task. Note that in figure 5.3, information about a chosen option is displayed. This option is the immediately previous choice. As soon as the bar was clicked, a slider appeared and was being

moved with the cursor while the sure amount was changed to the respective slider value.

Figure 5.3: Immediate Adjustment in the *IA* Task (Task 2)

Task No. 2

Choice Task

You chose **option A.**

A

1 - 20	21 - 100
£0	£12
20%	80%

B

1 - 100
£6
100%

Adjustment Task

Please choose a sure amount that you you find equally as attractive as option A.

Click on the bar below and drag the slider to set your equivalence amount:

£6.00

£0.00

£2.40

£4.80

£7.20

£9.60

£12.00

Once you are satisfied with your decision, please proceed to the next task.

Next Task

### 5.2.3 Task 3: $CE_{LA}$ through Adjustment Tasks following Choice Displays

After the binary choice trials were completed in Part 1, the program identified for which of the binary choices the participant did not show a 100% preference. Since each trial is repeated 5 times, there is always either a minority and majority choice in those cases.

In Part 2, the *IA* task (Task 2) was replaced by a *LA* task (Task 3), which instead featured a later adjustment. Participants saw a display of one of their past minority choices for each of the trials with no “unanimous” preference. They were then prompted to state their MSoP of that specific choice. As in the *IA* Task, participants had the option to state a CE that logically contradicts their past choice.

No information was given to the participant whether or not the past choice was atypical, e.g. 1 out of 5 times. So no problem of deception arose when showing a choice that was inconsistent with the participant’s average preference.

The same was repeated for all majority choices, including unanimous majority choices. Therefore, the number of trials of *IA* Tasks lies between the maximum of 22 (no choice repetitions show unanimous preference) and the minimum of 12 (all choice repetitions show unanimous preference, therefore 12 majority choice displays). If the display of a trial was prevented because of a lack of a minority choice, a distractor task was displayed instead.

As in the *IA* Task, the CE value in combination with the sure amount provides an MSoP but through a later adjustment,  $|CE_{LA} - SureAmount| = MSoP$ . This design allows us to discern between adjustments in which participants still were aware of their previous choice (*IA* Task) and adjustments in which participants only have information about one of their past choices (*LA* Task). *LA* Tasks thereby rule out any spill-over effects.

Note that in the *LA* Task, shown in figure 5.4, information about only a single choice is displayed. This information does depend on a previous choice, but unlike in the *IA* Task, the adjustment prompt does not immediately follow the choice. As soon as the slider is being moved, the sure amount is changed to the

respective slider value.

Figure 5.4: Later Adjustment (of a past sure amount) in the *IA* Task (Task 3)

Task No. 6

**Adjustment Task**

On a previous occasion, you chose **option A**.

Please click and adjust the slider for the sure amount until you find both options equally attractive.

A

1 - 20	21 - 100
£0	£12
20%	80%

B

1 - 100
£6
100%

Please choose a sure amount that you find equally as attractive as option A.

Click on the bar below and drag the slider to set your equivalence amount:

£6.00

|

£0.00
£2.40
£4.80
£7.20
£9.60
£12.00

Once you are satisfied with your decision, please proceed to the next task.

Next Task

#### 5.2.4 Task 4: $CE_{DV}$ through Direct Valuations

Task 4, the *DV* Task, involved standard CE elicitations for both the P-Bet and the \$-Bet. These tasks were repeated 5 times, totalling 10 valuation tasks. The repetition of valuations is not only necessary to achieve a better estimate of a participant's average valuation but also to allow an estimation of the valuations' variance. Par-



ticipants were prompted to choose a sure amount that they perceived to be equally as attractive as the lottery. This is the classic CE through direct valuation,  $CE_{DV}$ .

Note that in the *DV* Task, shown in figure 5.5, no information about previous choices is displayed. Participants only stated their preference by moving the slider. As soon as the slider was moved, the respective amount would appear in the second box instead of the question mark. Apart from this, the *DV* Task does not differ from the *IA* or *LA* Task. This is a deliberate design so all tasks appear as similar as possible.

Figure 5.5: Direct Valuation in *DV* Task (Task 4)

Task No. 5

Equivalence Task

Please choose a sure amount that you find equally as attractive as option A.

A

1 - 20	21 - 100
£0	£12
20%	80%

B

1 - 100
£?
100%

Click on the bar below and drag the slider to set your equivalence amount:

£0.00£2.40£4.80£7.20£9.60£12.00

Once you are satisfied with your decision, please proceed to the next task.

Next Task

### 5.3 Predictions

Based on the simulation results in chapters 3 and 4, the experiment’s data allows us to test a number of qualitative predictions. First, we test whether we can replicate a classic preference reversal and if this preference reversal also systematically occurs when using participants’ SI points instead of direct choices between P-Bet and \$-Bet. Second, we test whether the positive MSoP mismatch can be replicated for adjustments of sure amounts. Third, we test whether MSoP mismatches can be attributed to spill-over effects, consistency-seeking behaviour, or both.

Given the variability in preferences across participants, all generated variables will be tested on a within-individual basis and on accumulated information, i.e. means and medians, whenever possible. Since violations of procedure invariance are of interest in this experiment, within-individual data is compared across tasks.

The preference reversal occurs when comparing choice tasks with valuation tasks, therefore involving a between-task comparison of within-individual data, i.e. the average preference in choices vs. median preference in valuations. Data on MSoP mismatches is generated by a between-task comparison of individual data as well: the valuation from an MSoP task is compared to the participant’s SI point. And for comparing MSoP mismatches across trials, the extent of differences between participants’ MSoP and SI points are compared between *IA* and *LA* Tasks.

#### 5.3.1 Preference Reversal Replication

The experiment presupposes the classic preference reversal of preferring the P-Bet over the \$-Bet in direct choices while also valuing it lower than the \$-Bet in direct valuations ( $CE_{DV}$  from the *DV* Task) to establish that we are replicating the classic phenomenon. This reversal is also related to the SI points of each participant in the experiment.

### 5.3.1.1 Measures of Central Tendency

The predicted choices are probabilistic and an experiment can only feature a limited number of choices. Therefore, some participants might choose a P-Bet over a \$-Bet more often in the experiment even though they prefer the \$-Bet on average. If their valuations then reflect their underlying preference for the \$-Bet, this would result in a false positive for a preference reversal. It follows that we can only judge the likelihood of a preference reversal from the experimental data. But we predict that the results of Bostic et al. (1990) in the likelihood of preference reversals will be replicated. I.e., the likelihood of a preference reversal is higher when comparing reversals from direct choices to valuations than when comparing reversals from direct choices to SI points. In order to reduce noise in the analysis, we will compare the *average* preference in choices, i.e. the majority choice for either lottery, the *median* valuation, and the SI point separately for each participant.

#### Hypothesis 1:

Preference Reversals are more likely when comparing median  $CE_{DV}$  (*DV* Task Valuations) to average choice preference than when comparing  $CE_{SI}$  (*SI* Task SI points) to average choice preference.

### 5.3.2 MSoP Mismatch Replication

We will test if we replicate Butler et al.'s (2014a) findings for MSoP values following choices between lotteries and sure amounts. I.e., that participants' adjustments of sure amounts towards their SI points overshoot the SI point on average. This constitutes a positive MSoP mismatch. This means that for the average participant  $i$ ,  $CE_{IA,i}$  will be larger (smaller) than  $CE_{SI,i}$  following sure amounts smaller (larger) than  $CE_{SI,i}$ .

We predict that both spill-over and consistency-seeking behaviour effects exist, resulting in a positive MSoP mismatch in *IA* Tasks ( $CE_{IA}$ ) and *LA* Tasks ( $CE_{LA}$ ).

#### Hypothesis 2:

$$(CE_{IA} - CE_{SI})_i > 0 \text{ for sure amount} < CE_{SI,i}$$

and

$$(CE_{IA} - CE_{SI})_i < 0 \text{ for sure amount} > CE_{SI,i}$$

**Hypothesis 3:**

$$(CE_{LA} - CE_{SI})_i > 0 \text{ for sure amount} < CE_{SI,i}$$

and

$$(CE_{LA} - CE_{SI})_i < 0 \text{ for sure amount} > CE_{SI,i}$$

Note that hypothesis 3 only implies consistency-seeking behaviour, which results in a positive MSoP mismatch.

As explained in Section 5.1.1, consistency-seeking behaviour will have a similarly positive effect on MSoP as spill-over effects, only that it will occur in both immediate as well as delayed adjustment tasks. So if hypotheses 2 and/or 3 are rejected, consistency-seeking behaviour is ruled out as well.

**5.3.3 Separating Spill-over Effects and Consistency-Seeking Behaviour by comparing between *IA* and *LA* Tasks (Tasks 2 and 4)**

If hypotheses 2 and 3 are *both* true, the possibility still remains that spill-over effects do not exist and only consistency-seeking behaviour causes MSoP mismatches. The *LA* Task, which elicits  $CE_{LA}$ , is isolated from the previous choice over the lottery-sure amount pair. So the participant will not be influenced in their valuation by a readily available sample of mental evidence. If spill-over effects do exist, this mental sample will be available in the *IA* Task, which elicits  $CE_{IA}$ . If spill-over effects cause a mismatch in  $CE_{IA}$  values, the same mismatch will not be present in  $CE_{LA}$  values. Therefore, we assume that spill-over effects are only possible in an *IA* Task but not in a *LA* Task while effects from consistency-seeking behaviour occur in both.

As both effects positively influence the MSoP mismatch but are only both present in the *IA* Task, MSoP values in *IA* Tasks should show a stronger mismatch

than in *LA* Tasks. Therefore, we predict that the (positive) MSoP mismatch is larger in *IA* Tasks than in *LA* Tasks.

**Hypothesis 4:**

$$(CE_{IA} - CE_{SI})_i > (CE_{LA} - CE_{SI})_i > 0$$

*for sure amount*  $< CE_{SI,i}$

and

$$(CE_{IA} - CE_{SI})_i < (CE_{LA} - CE_{SI})_i < 0$$

*for sure amount*  $> CE_{SI,i}$

The combination of possible hypotheses offers a number of different interpretations based on the data. Some evidence from previous experiments already exists. Hypothesis 1 has already been established in the literature: The classic preference reversal phenomenon exists and is stronger between choices and valuations than between SI points and valuations (Bostic et al., 1990). Evidence for Hypothesis 2 has been documented by Butler et al. (2014a) for positive adjustments of a lottery's payoffs.

If no evidence in support of hypotheses 2, 3, and 4 is found, neither spill-over effects nor effects from consistency-seeking behaviour exist in measurable strength because no MSoP mismatch is found at all. But if an MSoP mismatch does exist, this mismatch could then be attributed either to spill-over effects or consistency-seeking behaviour. If the MSoP mismatch only exists in the *IA* Task (Hypothesis 2 only), only spill-over effects can be causal to the MSoP mismatch. If the MSoP mismatch has the same strength in *IA* and *LA* Tasks, only consistency-seeking behaviour can be causal to the MSoP mismatch (Hypothesis 2 and 3, but not 4). And if the MSoP mismatch exists across *IA* and *LA* Tasks but is strongest in *IA* Tasks, both spill-over effects as well as consistency-seeking behaviour are causal to the MSoP mismatch (Hypothesis 2, 3, and 4).

## 5.4 Results

### 5.4.1 Preference Reversals

As observed in the literature, average preferences switch from the P-Bet in choices to the \$-Bet in valuations. 70% of participants chose the P-Bet over the \$-Bet in a direct binary choice in the majority of cases. But 81% of participants' median *DV* Task valuations for the \$-Bet were higher than those for the P-Bet<sup>2</sup>, documenting the classic preference reversal.

Table 5.1 compares SI points ( $CE_{SI}$  from *SI* Tasks) to direct choices between lotteries. Table 5.2 compares medians of the direct valuations ( $CE_{DV}$  from *DV* Tasks) to these choices. Both comparisons show a number of preference reversals from preferring the P-Bet in direct choices to preferring the \$-Bet in inferred CEs. These occur in 12% of cases, when using *SI* Tasks. But in case of *DV* Task valuations, these are much more frequent: 53% of cases and 25 percentage points of the increase are due to participants that unanimously prefer the P-Bet in direct choices. Table 5.3 lists these observations. Comparing the likelihood of a preference reversal with a simple binomial test,  $H_0$  of equal probabilities of a preference reversal across *SI* and *DV* Tasks is rejected with  $p < 0.001$  in favour of hypothesis 1.

By comparing direct choices from *SI* Tasks with  $CE_{DV}$  values from *DV* Tasks, we established that 39 out of 74 participants showed the classic preference reversal. SI points for the P-Bet ( $mean = £7.07$ ,  $median = £7.16$ ,  $sd. = 1.51$ ) and the \$-Bet ( $mean = £7.00$ ,  $median = £7.16$ ,  $sd. = 1.63$ ) do not statistically significantly differ (Wilcoxon signed rank test:  $V=17$ ;  $p=0.21$ ). But Valuation medians of participants in *DV* Tasks do statistically significantly differ between the P-Bet ( $mean = £8.08$ ,  $median = £8.60$ ,  $sd. = 1.68$ ) and the \$-Bet ( $mean = £15.87$ ,  $median = £13.70$ ,  $sd. = 9.04$ ), according to a Wilcoxon signed rank test ( $V=2,555$ ;  $p < 0.001$ ).

Note that 64.9% of participants show a unanimous preferences in repeated

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<sup>2</sup> $H_0$  of similar probabilities of preferring the P-Bet is rejected by a binomial test with  $p < 0.001$ .

Table 5.1: Cross Tabulation of Preference through Direct Choice (*SI* Tasks) and SI Points (*SI* Tasks)

		Difference in $CE$ Values inferred from Choices ( <i>SI</i> Task)			
Choice Share ( <i>SI</i> Task)		$CE_{SI}(P) > CE_{SI}(\$)$	$CE_{SI}(P) = CE_{SI}(\$)$	$CE_{SI}(P) < CE_{SI}(\$)$	count
	P-Bet vs. \$-Bet				
	5P / 0\$	30	1	2	<b>33</b>
	4P / 1\$	5	4	5	<b>14</b>
	3P / 2\$	3	0	2	<b>5</b>
	2P / 3\$	0	0	4	<b>4</b>
	1P / 4\$	0	0	3	<b>3</b>
	0P / 5\$	0	1	14	<b>15</b>
	count	<b>38</b>	<b>6</b>	<b>30</b>	<b>74</b>

Preference reversals shaded in red. Consistent preference relations shaded in green.

P vs. \$ choices (44.5% for the P-Bet and 18.9% for the \$-Bet). But only 35% of participants have a unanimous choice preference that is both reflected in direct choices as well as SI points. This is similar to the results observed by Bostic et al. (1990) and Butler and Loomes (2007).

Table 5.2: Cross Tabulation of Preference through Direct Choice (*SI* Tasks) and Direct Valuation (*DV* Tasks)

		Difference in median $CE$ Values ( $DV$ Task)				
Choice Share ( $SI$ Task)	P-Bet vs. \$-Bet	$med(CE_{DV}(P)) > med(CE_{DV}(\$))$	$med(CE_{DV}(P)) = med(CE_{DV}(\$))$	$med(CE_{DV}(P)) < med(CE_{DV}(\$))$	count	
		5P / 0\$	11	2	20	<b>33</b>
		4P / 1\$	0	0	14	<b>14</b>
		3P / 2\$	0	0	5	<b>5</b>
		2P / 3\$	0	0	4	<b>4</b>
		1P / 4\$	0	0	3	<b>3</b>
		0P / 5\$	0	0	15	<b>15</b>
		count	<b>11</b>	<b>2</b>	<b>61</b>	<b>74</b>

Preference reversals shaded in red. Consistent preference relations shaded in green.

Table 5.3: Cross-Tabulation of Preference Reversals between direct Choices and SI points (*SI* Tasks) vs. direct Choices and Valuations (*DV* Tasks)

CE Elicitation Method				
Reversals from P-Bet vs. \$-Bet Choice to CE		SI Task: SI Point	DV Task: Direct Valuation	count
	$P \rightarrow \$$	9	39	<b>48</b>
	$\$ \rightarrow P$	0	0	<b>0</b>
	count	<b>9</b>	<b>39</b>	<b>48</b>



## 5.4.2 MSoP Mismatches

### 5.4.2.1 *IA* Tasks: Choose&Adjust

Average valuations in *IA* Tasks also exceed the SI points of both lotteries (see figure 5.8). This already shows that any aggregate MSoP mismatch is not negative. But this data does not take into account the individual level. As participants differ in their SI points, we will need to compare their valuations from *IA* Tasks to their individual SI points. Comparing  $CE_{IA}$  values to  $CE_{SI}$  SI points from *SI* Tasks then allows us to identify MSoP mismatches.

If a participant has an SI point  $CE_{SI}$  of £10 for a lottery and has rejected a sure amount of £2 in favour of the lottery in the *SI* Task, the participants' *choices* would suggest that the participant values the lottery £8 higher than the sure amount on average. This can be checked by comparing the  $CE_{IA}$  values from *IA* Tasks to the same participant's SI point. If the example shows on average  $CE_{IA} > CE_{SI}$ , the participant will have a positive MSoP mismatch.

But adjustments can also be negative, when a participant has chosen a sure amount over a lottery. If in that case on average  $CE_{IA} > CE_{SI}$ , the participant shows a *negative* MSoP mismatch since the adjustment is not large enough for the sure amount to be equal to the participant's SI point.

To make these values comparable, we will need to measure the "excess" MSoP. I.e., how much a participant's  $CE_{IA}$  value overshoots their respective  $CE_{SI}$  value, contingent on the participant's adjustment being positive or negative. Therefore, a consistently positive excess in MSoP is equivalent to a positive MSoP mismatch and a consistently negative excess MSoP is equivalent to a negative MSoP mismatch.

See figures 5.6 and 5.7 for box plots of the differences in participants' mean difference between SI points and MSoP values for the P-Bet and the \$-Bet, separated by upward and downward adjustments as well as *IA* and *LA* Task types. Similarly for the P-Bet as well as the \$-Bet, these values differ in their sign dependent on the direction of the adjustment. Sure amounts are consistently adjusted upwards

too strongly and adjusted downwards too weakly to be consistent with the SI point. While the majority of positive adjustments overshoots the SI point, the majority of negative adjustments undershoots it.

For both lotteries, this results in a positive MSoP mismatch for positive adjustments and in a negative MSoP mismatch for negative adjustments. Mismatches in all four cases are highly significant. A Wilcoxon signed rank test rejects  $H_0$  of no MSoP mismatch in the positive adjustment (P-Bet:  $V=84,475$ ;  $p<0.001$ ; \$-Bet:  $V=105,340$ ;  $p<0.001$ ) and in the negative adjustment case (P-Bet:  $V=6,802$ ;  $p<0.001$ ; \$-Bet:  $V=5604.5$ ;  $p<0.001$ )<sup>3</sup>.

Since MSoP mismatches are positive for upward adjustments, the data cannot reject the corresponding null hypothesis to hypothesis 2, finding no evidence for an MSoP mismatch that is always positive. However, the MSoP mismatch is replicated for positive adjustments and a previously unknown negative MSoP mismatch is found for negative adjustments.

This data replicates the positive MSoP mismatch documented in upward lottery adjustments by Butler et al. (2014a) for sure amounts. As Butler et al. only measured positive adjustments in lotteries, it remains unclear if negative *lottery* adjustments also result in a negative MSoP mismatch.

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<sup>3</sup>Note that a particularly high Wilcoxon test statistic can occur with higher sample sizes because a test statistic that rejects  $H_0$  rises exponentially in sample size (Agresti, 2003; p.301). The experiment features 2 main lotteries, 5 non-zero sure amounts, and 2 adjustments per lottery/non-zero sure amount pair. Given 74 participants, this produces 1,480 MSoP observations, which makes such high test statistics more likely.

Figure 5.6: *IA* and *LA* Tasks: Box plots of excess MSoP values for the P-Bet

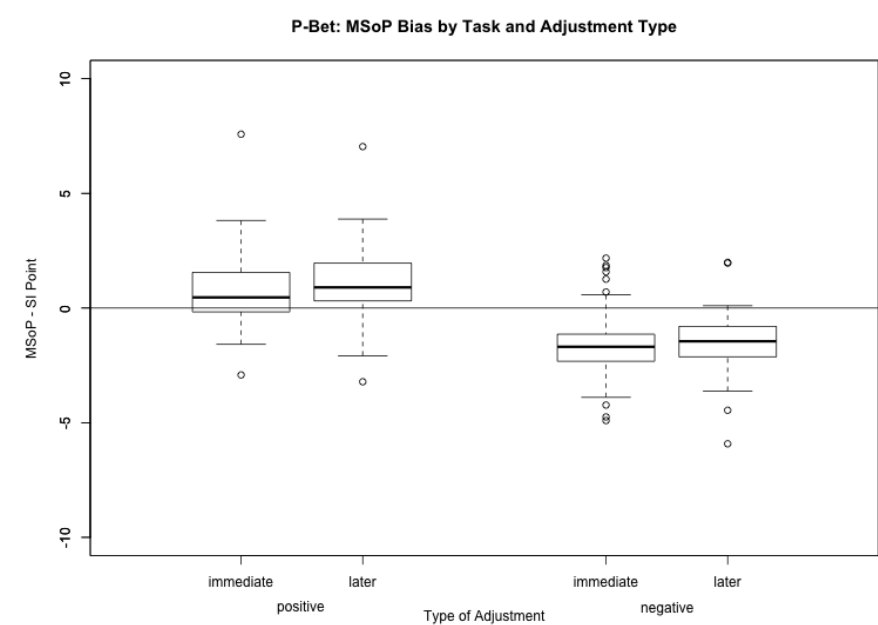
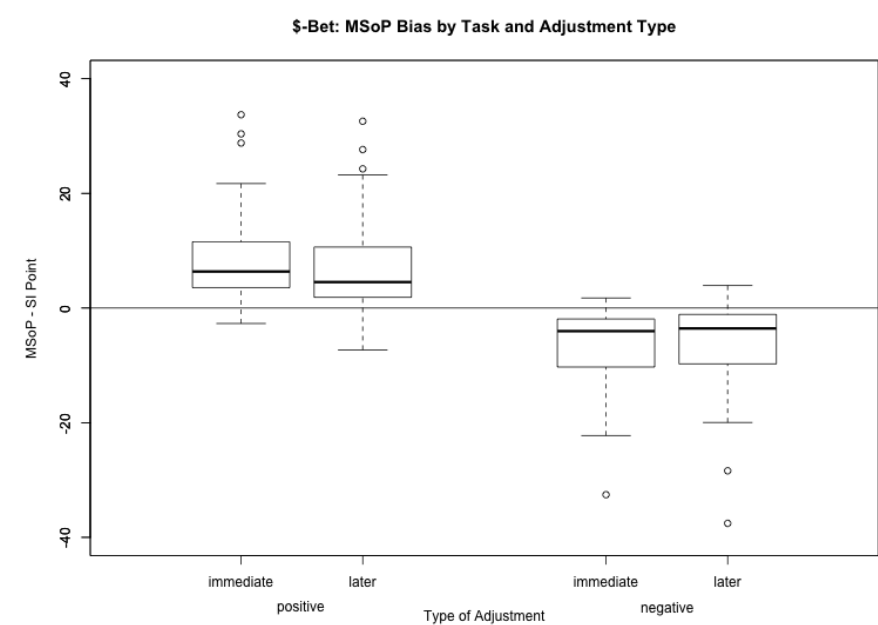


Figure 5.7: *IA* and *LA* Tasks: Box plots of excess MSoP values for the \$-Bet



#### 5.4.2.2 *LA* Tasks: Choose&Adjust

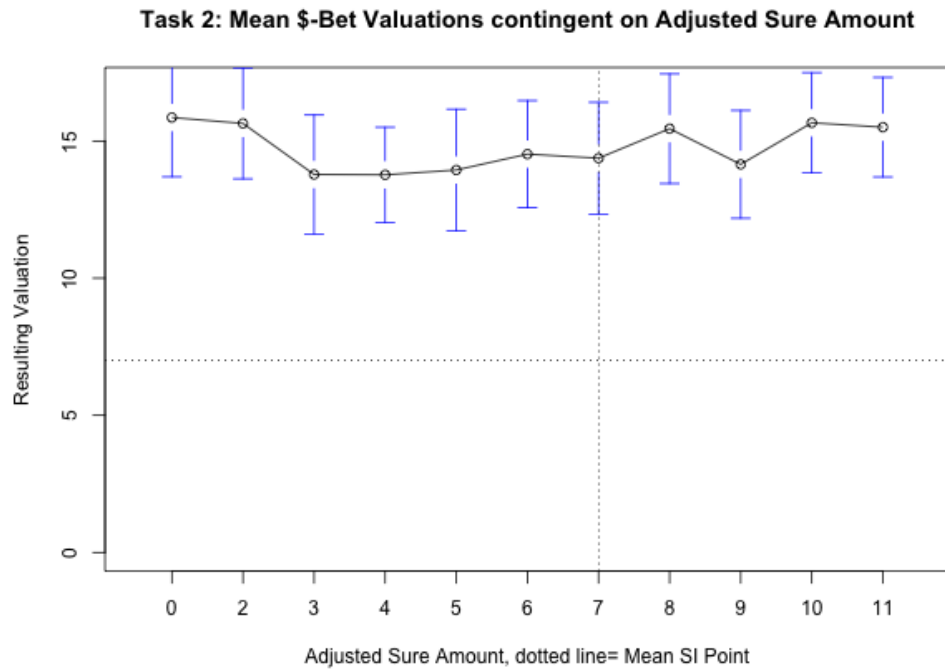
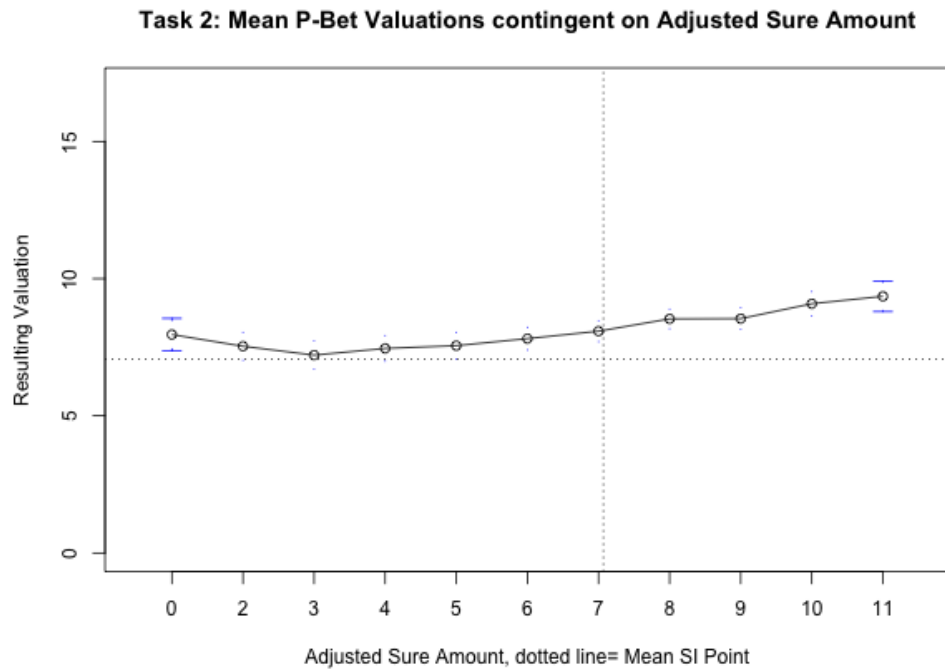
With the same method, we can also calculate possible MSoP mismatches in  $CE_{LA}$  in *LA* Tasks, also displayed in the box plots in figures 5.6 and 5.7. As in *IA* Tasks, the same pattern of a positive MSoP mismatch for upward adjustments and a negative MSoP mismatch for downward adjustments emerges. Again, all mismatches are highly significant and a Wilcoxon signed rank test rejects  $H_0$  of no MSoP mismatch in the positive adjustment (P-Bet:  $V=28,588$ ;  $p<0.001$ ;  $\$$ -Bet:  $V=28,662$ ;  $p<0.001$ ) and in the negative adjustment case (P-Bet:  $V=2,166$ ;  $p<0.001$ ;  $\$$ -Bet:  $V=3,574$ ;  $p<0.001$ ). Similar to *IA* Tasks, the data cannot reject the corresponding null hypothesis to hypothesis 3. Therefore, evidence for MSoP mismatches does exist but it does not support the exact specification of hypothesis 3.

#### 5.4.2.3 Possible Anchoring Effects and SI Point Effects

In the experiment, *IA* Tasks display a variety of starting values to be adjusted by participants for a valuation. Since a specific sure amount is displayed just before a participant's valuation and was also featured in a choice process, it might be possible that this sure amount serves as an anchor in the subsequent valuation process. A sufficiently high anchor might then cause valuations to be too high. For upward adjustments, this would mean that valuations are consistently too high and overshoot the SI point, resulting in a positive MSoP mismatch. Similarly for downward adjustments, valuations would undershoot and not be low enough to match the SI point, resulting in a negative MSoP mismatch.

While the pattern of MSoP mismatches is the same, there is only limited evidence for anchoring effects (see figure 5.8, with bars denoting 95% confidence intervals). If an anchoring effect exists that prevents valuations from reaching the SI point, these valuations should on average appear between the SI point and sure amount that needed to be adjusted. But a large majority of  $CE_{IA}$  valuations that were elicited in the experiment are above the SI point (shown by the dotted lines in the figure) even though initial sure amounts were below it.

Figure 5.8: *IA* Task: Mean Valuations dependent on offered Sure Amount



There is a highly statistically significant anchoring effect of starting values on P-Bet valuations but it is not sufficiently strong to explain the MSoP mismatches. If MSoP mismatches result from this effect, average valuations should be below the SI point for sure amounts below the SI point.

A linear regression does report an increase of the dependent variable **valuation** of £0.17 with a £1 increase in the independent starting value **sure\_amount**. In the case of the P-Bet, the regression has an intercept at £7.13, already above the P-Bet's average SI point of £7.07 (See table 5.4). Any valuation for any starting value is therefore an overvaluation relative to the P-Bet's SI point. Therefore, if a starting value for the P-Bet is £1 closer to the participant's SI point, final valuations increase by £0.17 in the model. But this means that only sure amounts below zero could result in subsequent positive adjustments that undershoot the SI point. Consequently, if an anchoring effect exists in *IA* Tasks for the P-Bet, the positive MSoP mismatch would exist even without the anchor.

Similarly, \$-Bets are always overvalued in relation to the SI point of £7.00 since the lowest predicted valuation is at £14.69 for a starting value of £0 (See table 5.5). The anchoring effect of starting values on \$-Bet valuations is even weaker. For a £1 increase in the starting value **sure\_amount**, the dependent variable **valuation** only increases by £0.01 and this effect does not reach statistical significance. Therefore, we cannot reject  $H_0$  and conclude that there is no sufficient evidence for anchoring effects on \$-Bet valuations in *IA* Tasks.

Table 5.4: Linear regression model of sure amounts on final P-Bet valuations in *IA* Tasks

Independent Variable	Coefficient			
	Estimate	Std. Error	t-Value	P(> t )
Intercept	7.130	0.147	48.668	< 2E-16 ***
sure_amount	0.165	0.022	7.617	7.22e-14 ***
Residual std. error: 2.047 on 812 degrees of freedom				
Multiple R-squared: 0.067				
Adj. R-squared: 0.066				
F-statistic: 58.02 on 1 and 812 degrees of freedom				
p-value: 7.21e-14				
Signif. codes: 0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1				

Table 5.5: Linear regression model of sure amounts on final \$-Bet valuations in *IA* Tasks

Independent Variable	Coefficient			
	Estimate	Std. Error	t-Value	P(> t )
Intercept	14.690	0.615	23.890	< 2E-16 ***
sure_amount	0.0176	0.091	0.194	0.846
Residual std. error:	8.593 on 812 degrees of freedom			
Multiple R-squared:	4.63e-05			
Adj. R-squared:	-0.001			
F-statistic:	0.038 on 1 and 812 degrees of freedom			
p-value:	0.846			
Signif. codes: 0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1				

### 5.4.3 Spill-over Effects and Consistency-Seeking Behaviour

#### 5.4.3.1 Comparison of *IA* and *LA* Tasks

Valuations in *LA* Tasks are not immediately preceded by a choice between the displayed lottery and starting value. The fact that MSoP mismatches were detected in  $CE_{LA}$  values in *LA* Tasks shows that spill-over effects from a preceding choice cannot be the sole driver of these mismatches.

Any difference in MSoP mismatches between *IA* and *LA* Tasks can only be explained by the difference between the nature of the task. This difference is the length of delay between choice and adjustment, resulting in the possibility of spill-over effects in the *IA* Task. So we assume that any difference in MSoP mismatches between these task types exists because of a spill-over of mental evidence. If there is no difference, we will reject the conclusion that a spill-over of mental evidence causes MSoP mismatches. Since anchoring effects cannot explain MSoP mismatches either, this would then leave consistency-seeking behaviour as the remaining explanation for MSoP mismatches without altering the assumptions made for a sequential sampling process of accumulating mental evidence.

The number of MSoP observations in *LA* Tasks differs across participants. For participants who showed a unanimous preference towards a sure amount / lottery pair, the display of a minority choice was not possible and a distractor task was displayed instead. Therefore, weighing all MSoP observations the same for a comparison would overcount observations from participants with higher variance in their choices. The same applies to only comparing *IA* Tasks to *LA* Tasks where sure amount / lottery pairs and participants were the same.

MSoP observations in *IA* Tasks are unbalanced as well. Since only 2 out of 5 binary choices were randomly followed by an *IA* Task, only a minority of MSoP data covered both positive *IA* Task adjustments as well as negative *IA* Task adjustments for the same sure amount / lottery pair for the same participant.

Section 5.4.2 also shows that a difference exists between MSoP mismatches depending on the direction of adjustment. Not accounting for an uneven occurrence



of positive / negative adjustments could then attribute any difference between *IA* Tasks and *LA* Tasks to the direction of adjustment and not the task.

Therefore, we compare within-individual mean MSoP mismatches separately for positive and negative adjustments, solving the described issues in overcounting. Furthermore, these comparisons will also be separate for the P-Bet and the \$-Bet, yielding  $2 \times 2 = 4$  comparisons in total.

As we are testing for differences repeatedly, the probability of falsely identifying a statistically significant difference is increased<sup>4</sup>. Nonetheless, a Wilcoxon signed rank test does not report a statistically significant difference in any of the comparisons between *IA* Tasks and *LA* tasks. The effect in differences is strongest and almost significant when comparing positive *IA* and *LA* adjustments for the P-Bet ( $V=2,255$ ;  $p=0.06$ ), with the difference between positive *IA* and *LA* adjustments for the \$-Bet being the second-strongest ( $V=2,977$ ;  $p=0.12$ ). The same pattern of a stronger effect for P-Bet MSoPs also occurs when comparing negative *IA* and *LA* adjustments for the P-Bet ( $V=2,374$ ;  $p=0.32$ ), with the difference between negative *IA* and *LA* adjustments for the \$-Bet ( $V=1,620$ ;  $p=0.44$ ).

It follows that the corresponding  $H_0$  of equality of distributions to hypothesis 4 cannot be rejected. This leaves the account of consistency-seeking behaviour as the only explanation for the MSoP mismatches. But since hypotheses 2 and 3 were only accepted for positive adjustments, we cannot conclude with a clear explanation of the MSoP mismatch from existing theory. Specifically, the BREUT valuation model strictly rules out a negative MSoP mismatch in the case of consistency-seeking behaviour.

## 5.5 Contribution and Discussion

In a controlled experiment, Butler et al.'s (2014a) observation of a positive MSoP mismatch in contingent valuations was replicated for positive adjustments of sure amounts only. Negative adjustments of sure amounts were associated with a negative

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<sup>4</sup>E.g., at a significance level of  $\alpha = 5\%$ , the probability of a false positive occurring after 4 repeated tests equals  $\sum_{i=1}^4 (1 - \alpha)^{i-1} \alpha = 17.6\%$ .

MSoP mismatch.

By computing SI points for participants, we also established that initial sure amounts for the valuation of a P-Bet and a \$-Bet did not control a preference reversal between valuations and choices. Both the SVM mechanism as well as the SP model predict a preference reversal by assuming that DMs are subject to an anchoring effect. These models assume that an initial sure amount is incompletely adjusted from an anchor value towards the SI point. P-Bets are anchored below their SI point while \$-Bets are anchored above it. This results in an undervaluation of the P-Bet and an overvaluation of the \$-Bet (relative to their SI points), which can result in a preference reversal. But in our experiment both the P-Bet and the \$-Bet were systematically valued above their respective SI points in the experiment, even when being anchored below them.

The experiment rules out spill-over effects as a cause of the MSoP mismatch. No difference in MSoP values was found between immediate and delayed MSoP values. Therefore, we conclude that consistency-seeking behaviour in the form of cognitive dissonance is the remaining explanation for the described patterns in MSoP mismatches. But consistency-seeking behaviour does not provide a full explanation of the observed effects either. Downward adjustments of sure amounts consistently undershoot participants' SI points while upward adjustments overshoot it. If consistency-seeking behaviour introduces a positive effect on MSoP size for negative adjustments, at the very least this effect must have been outweighed by a tendency to overvalue a lottery relative to the SI point.

There is also no formal lottery valuation model that can make quantitative predictions of lottery valuations which could explain the preference reversal along with this two-fold pattern of MSoP mismatches. Explaining this interplay between choice and valuation will require a model that can explain not only the preference reversal but also the counterintuitive effect of consistency-seeking behaviour in MSoP valuations.

## Chapter 6

# Reaction Time Effects in Valuation Tasks

### 6.1 Introduction

Chapter 5 described how in an experiment, participants stated CE values for lotteries that were consistently above their SI points. The BREUT valuation model did not predict the correct effect directions for MSoP values and underpredicted the effect strength of the preference reversal phenomenon. But the qualitative prediction of direct lottery valuations above the respective SI points as the driving force of the preference reversal was correct. This chapter will investigate at the individual level what effect longer reaction times can have on direct valuations and how this relates to the preference reversal phenomenon.

Chapters 3 and 4 describe how the preference reversal phenomenon can be explained by DMs being boundedly rational: With unlimited deliberation time for their decisions, they will behave perfectly consistently across different procedures but constraints in mental resources and time keep them from achieving this. Their behaviour remains probabilistic and procedure-dependent but with longer deliberation times, inconsistencies between procedures decrease. That is to say, when everything else is equal, a DM that takes longer for a valuation will report a lottery CE that is less likely to be inconsistent with her underlying distribution of prefer-

ences. Do note, however, that this only applies if the process converges towards a core model that does not differ across procedures.

Some indicative evidence has already been generated. As explained in the literature review, Braga et al. (2009) that the number of preference reversals decreases when participants experience losses from playing out lotteries. But this need not imply that the same is the case when participants do not experience feedback and only deliberate for longer about lotteries. And failures to reduce biases in individually-longer reaction times have already been measured in an experiment using a different valuation task: Ashby et al. (2012) imposed time limits on participants' WTA/WTP elicitations for lotteries and found that endowment effects increase when allowing participants more time to deliberate.

So it would make sense to test whether participants' valuations actually converge towards a value that corresponds to an underlying preference. Blavatskyy and Köhler (2009a) show that convergence can occur in a valuation task. They incentivised participants to state their minimum selling price for a lottery at every moment throughout a valuation process. This was done by randomly terminating the pricing task and paying nothing in cases where an initial valuation was not reported. If a price was stated, they determined the payoff through the BDM procedure. Therefore, participants were incentivised to quickly state and subsequently update their reported price throughout their deliberation time. Blavatskyy and Köhler found that prices did not change randomly but instead converged towards a final valuation in the majority of cases (in accordance with their SP model). But they did not measure participants' SI points of the lotteries. Therefore, it is impossible to say from their experiment whether prices converged towards SI points.

The experiment in chapter 5 collected participants' valuations and also allowed us to infer SI points. So the data enables us to test if differences between valuations and SI points decrease in size and/or frequency for longer deliberation times. This gives rise to two questions:

- 1) How can this difference be measured? and
- 2) How can effects of longer deliberation times on this difference be measured?

Chapter 5 already addressed the first question of measuring differences between valuations and SI points, suggesting that valuations systematically differ from SI points. If we assume that something like a “true preference” manifests itself in the SI point, we can define this discrepancy as the absolute difference between a direct valuation and the participant’s SI point (see section 6.2.2 for the calculation logic of the SI point).

But how can an experiment measure the effect of deliberation time on this valuation distance? In the SVM mechanism and the BREUT valuation model, deliberation times of a DM are endogenous and vary across repetitions of the same decision task. Factoring out any other sources of noise in an experiment, this would predict that individually-longer reaction times can then be attributed to a longer mental process.

When combined with CE values, the marginal effect of an increase (i.e., a positive deviation) in deliberation time then becomes observable. And if these marginal effects on valuations are not in line with sequential sampling predictions from chapters 3 and 4, the fundamental sequential sampling assumption of the described models must be rejected for valuation tasks.

Section 6.1.1 provides a motivation for testing the hypothesis, section 6.2 describes the data from the experiment and the methods used, section 6.3 describes the predictions, and section 6.4 shows the results. Section 6.5 concludes with a discussion.

### **6.1.1 The Sequential Sampling Paradigm applied to Valuation**

As explained in more detail in chapter 2, Moffatt (2005) already established that participants in an experiment took relatively longer for choice tasks that were estimated to be similar in attractiveness to them. This suggests that as their underlying preference between lotteries becomes harder to identify, participants invest more cognitive effort into their choice. Note that this still requires an upper limit to avoid an infinite amount of effort in case of identical alternatives.

The common theme in a majority of sequential sampling models is that DMs

satisfice. I.e., they will sample just long enough to reach a “good enough” set of evidence that suggests which decision to make (Busemeyer, 2014). This reflects a speed-accuracy trade-off: As they are unwilling to invest too much time and effort into their decision, they are willing to accept a less precise estimate of which option is truly more attractive to them.

These models share the common component that a DM samples mental evidence that is distributed around her average preference, generating samples that also exhibit variance. This ensures that more sampling tends to lead to a more accurate estimation of an option’s attractiveness to the DM. But at the same time, an accumulated sample will never be sufficiently large to represent underlying preferences with complete accuracy. As all samples are subject to noise, some variability will always remain. But despite this, the representativeness of the sample increases in sample size.

#### **6.1.1.1 The BREUT Valuation Model**

Chapter 4 (section 4.3.3.3) concludes with a qualitative prediction of the BREUT valuation model: Everything else equal, a DM reports average valuations that are closer to the SI point when the DM’s mental sample is larger. While some valuations lie above the SI point and some lie below it. But the dispersion of valuations around the SI point is greater for smaller samples. This effect is also stronger for the \$-Bet. Valuations are also more likely to be above the SI point for smaller samples, leading to overvaluation that results in the preference reversal phenomenon. So if SI points between the lotteries do not differ, average \$-Bet valuations are more likely to be above P-Bet valuations for small samples.

If more sampling is associated with individually-longer reaction times, this line of reasoning would predict that individually-longer reaction times would lead to smaller differences between valuations and SI points. So the BREUT valuation model predicts larger valuation differences for quicker responses and lower valuation differences for longer responses. When P-Bets are chosen over \$-Bets more than 50% of times as in the experiment reported in chapter 5, BREUT predicts that the

P-Bet’s SI point is greater than the \$-Bet’s SI point. In that case, the model also predicts that \$-Bet valuations are more likely to be smaller than P-Bet valuations for longer deliberation times because both lotteries’ valuations. Average lottery valuations both start off above the SI point and converge downwards. But as the effect is stronger for the \$-Bet and the \$-Bet’s SI point is also below the P-Bet’s SI point, the reduction of \$-Bet valuations is stronger and “overtakes” the reduction in P-Bet valuations.

But how can we measure if a process goes on “for longer”? The solution to this lies at the level of participants. We assume that participants’ characteristics, which are represented by the underlying BREUT parameters, do not change throughout the experiment. Although we cannot observe these parameters, we can observe the effects of differences in reaction time on participant behaviour. Also, as explained in chapter 2 (section 2.5.1), we assume that within-individual differences in reaction times of experimental tasks can be used as a proxy for differences in deliberation time of the participant. So we observe how participants differ in their preference when reaction times are longer or shorter than usual.

#### **6.1.1.2 The SVM Mechanism**

Chapter 3 (section 3.3.5) explains how the SVM mechanism predicts that candidate values converge towards the SI point throughout the valuation process for all lotteries. As the process randomly stops and settles on candidate values as final valuations, the marginal effect is that all lottery valuations are closer to the respective SI point for longer deliberation times. Again, we assume that longer deliberation times are reflected in longer reaction times in the experiment.

The SVM mechanism assumes that starting values are below the SI point for the P-Bet and above the SI point for the \$-Bet. But in the case of average responses in the experiment, starting values must have been above the SI points both for the P-Bet and the \$-Bet because final valuations were above the SI points. So in order to assume that the SVM mechanism is correct, we need to alter this assumption so that starting values are above SI points for both lotteries. This then resembles Tversky

and Kahneman’s (1974) “Anchoring and Adjustment” approach. Then, the SVM mechanism prediction of marginal effects is the same as for the BREUT valuation model: Lottery valuations start off above the SI point and then converge towards it for longer reaction times. Therefore, absolute differences between valuations and SI points decrease for longer reaction times.

## 6.2 Data

### 6.2.1 Direct Lottery Valuations

We use the data from the experiment described in chapter 5. Again, the 3 of the 77 participants that showed non-compliance with the incentivisation procedure were excluded from the analysis (see section A.2 in the appendix).

Each participant completed 5 repetitions of direct valuation tasks, both for the P-Bet and the \$-Bet (see figure 5.5 in chapter 5 for the task display). The P-Bet featured an 80% chance of winning £12 and the \$-Bet featured a 25% chance of winning £50. Every task provides a valuation  $Valuation_{it}$  for participant  $i$  in trial  $t$ . In combination with the SI point, this also provides an estimated deviation from the SI point:  $|Valuation_{it} - SI\ Point_i|$ .

### 6.2.2 Measuring Preferences through Choices

The choice part of the experiment involved binary choices between a lottery and a sure amount (see figure 5.2 in chapter 5 for the task display). From this data, individual SI points were inferred through the same counting method described in section 5.2.1 in chapter 5. Participants chose between playing out the P-Bet and 6 different sure amounts with two groups either only facing even  $\{0; 2; 4; \dots; 10\}$  or odd  $\{0; 3; 5; \dots; 11\}$  numbers (but both times including zero). Similarly, the \$-Bet was displayed alongside the same sure amounts. All trials but the single choices against £0 were repeated 5 times, so SI points were based on 26 choices between a lottery and a sure amount for each individual and lottery. In addition, each participant faced 5 binary choices between the P-Bet and the \$-Bet.



### 6.2.3 Reaction Times

The average reaction time over 740 direct valuation tasks was 9.13 seconds (median=9.01, sd=0.60). Of these, 370 tasks considered the P-Bet and the \$-Bet each.

#### 6.2.3.1 Within-Individual Reaction Times

Following Luce (1986), the reaction time in a trial is assumed to be equal to 1) deliberation time plus 2) time to understand and subsequently respond to the task, 3) possible effects of experience on the reaction time, and 4) experimental noise.

**Deliberation Time** Variations in deliberation time are the variable of interest, being analysed for possible effects on the difference between valuation and SI point. But participants might vary in their mental processing speed, which in theory corresponds to a speed of accumulating a mental sample. So one participant might be affected more by an increase in their deliberation time. We can avoid these confounding effects by only testing for individual marginal effects of reaction times. I.e., to estimate the statistical effect of a one-unit increase in reaction time on the size of the mismatch at the level of the individual.

Thereby, we limit ourselves to testing a qualitative hypothesis towards the direction of a statistical effect. This has the advantage that we filter out individual-specific effects that might affect the results without the need to infer exact minimum and maximum values for deliberation times.

**Understanding and Executing a Task** The time participants need to identify a task and to subsequently execute their choice provides a lower bound for reaction times. A crucial assumption for this analysis to work is that this individual-specific lower bound does not change throughout the experiment (following Luce, 1986; and Ratcliff and Tuerlinckx, 2002). Any additional time that the participant might need and that changes throughout the experiment will be attributed to an experience effect.

**Experience** In order to get a clearer estimate of the effect of reaction times, we also control for the effect of experience on reaction times. Participants might get quicker at stating their valuation as the experiment goes on because participants will become more familiar with completing the task.

Reaction times are assumed to decrease exponentially towards a lower limit throughout the experiment and this is modelled by adding a regressor equivalent to the inverse of the task order. I.e., we assume that individuals take more time to respond at the start of the experiment. Participants will on average respond quicker with each subsequent trial but this effect decreases in time and never moves below an individual-specific lower bound.

**Noise** The remainder of reaction times will be attributed to idiosyncratic noise. Lastly, we use a  $\log_{10}$  transformation on reaction times to avoid issues with non-normality and outliers. Reaction times in experiments are typically distributed with a positive skew and a lower but no theoretical upper bound (for a fundamental discussion of this, see e.g. Luce, 1986). Transforming reaction time data this way will approximate a normal distribution, resulting in symmetrical estimation errors, and lessens the impact of outliers without the need to exclude parts of the data (Whelan, 2008; Ratcliff, 1993). See figure A.4 in the appendix for histograms of reaction times before and after the log-transformation.

#### **6.2.3.2 Exogenous Effects on Reaction Times**

The direct valuation tasks require participants to move a slider until they find a value that makes the sure amount as attractive as the displayed lottery to them. The nature of this elicitation might make valuations more or less precise because of different levels of difficulty. This could be the lottery range, which affects the range of the possible slider values (the P-Bet ranges from £0 to £12 while the \$-Bet ranges from £0 to £50). Sliders have the same absolute size on the interface for all tasks. So a larger range makes it more difficult for the participant to select their CE via the slider. So to avoid confounding effects, analyses are carried out separately for

the P-Bet and the \$-Bet.

In short, we compare direct valuation tasks where the only difference between them was the trial order in the experiment. Since the tasks are identical and the participants are the same, the only remaining non-idiosyncratic noise will be the level of experience of the individual, which is covered by the regressor for experience effects.

### 6.2.3.3 Replicating Moffatt's (2005) Results

The choice data also allows us to test whether Moffatt's (2005) results are replicated for choices between lotteries and sure amounts in this experiment. Moffatt found that participants' reaction times increased for binary choices when the estimated difference in attractiveness between options was smaller. We can test if this also applies to choices closer to the SI point. If reaction times are distributed in line with Moffatt's findings, this suggests that other reaction times in the experiment are less likely the result of external sources of noise.

While Moffatt estimated difference in attractiveness through a utility difference between options, we can use a simpler approach. We can use the absolute difference between a lottery's SI point and the offered sure amount as a measure for the difference in attractiveness. As we only run the regression separately for P-Bets and \$-Bets, we also do not need regressors for lottery complexity, and expected lottery payoff difference. Also, we only test for a potential non-linear effect direction to keep the number of regressors low because there are only 26 observations per participant. So we only use this simplified regression equation for each participant  $i$ :

$$RT_{it} = \beta_{i0} + \beta_{i1} SI\ Distance_{it} + SI\ Distance_{it}^2 + \beta_{i2} Trial\ Index_{it}^{-1} + \varepsilon_{it}$$

for individuals  $i = 1, \dots, 74$  and trials  $t = 1, \dots, 26$ ,

with an intercept  $\beta_{0i}$ ,

where  $RT_{it}$  is the log10-transformed reaction time of individual  $i$  in trial  $t$ ,

where  $SI\ Distance_{it}$  is the absolute difference between individual  $i$ 's SI point and the offered sure amount in trial  $t$ ,

where  $SI\ Distance_{it}^2$  is this value squared to capture non-linear effects,  
where  $Trial\ Index_{it}^{-1}$  is the inverse of the trial index,  
and error term  $\varepsilon_{it}$ .

The average reaction time over 1924 choice tasks was 8.34 seconds for the P-Bet (median=8.26, sd=0.52) and 8.39 seconds for the \$-Bet (median=8.28, sd=0.56). Mean SI points were £7.07 for the P-Bet (median=£7.16, sd=1.51) and £7.00 for the \$-Bet (median=7.16, sd=1.63). Table 6.1 shows the regression results for the P-Bet and the \$-Bet. Note that all effects sizes apply to log10-transformed reaction times.

For the P-Bet,  $SI\ Distance$  has a small negative effect on reaction times. Together with the smaller positive  $SI\ Distance_{it}^2$  regressor, distance to the SI point has negative non-linear effect on reaction time and becomes weaker for greater distance. Together, for a £1 increase at the SI point in difference between the sure amount and the lottery, the mean reaction time of 8.34 seconds decreases by 0.11 seconds. In the most extreme case of £0 as sure amount, the non-linear effect results in a predicted reaction time of 7.91 seconds.  $trial^{-1}$  also shows an effect of experience: An average choice in the first trial is 2.81 seconds longer due to experience effects that subsequently decrease throughout the experiment.

For the \$-Bet,  $SI\ Distance$  also has a negative effect on reaction times, with a smaller positive  $SI\ Distance_{it}^2$  regressor: For a £1 increase, the mean reaction time of 8.39 seconds decreases by 0.02 seconds. In the case against £0, the non-linear effect results in a predicted reaction time of 8.27 seconds.  $trial^{-1}$  shows a similar effect: An average choice in the first trial is 1.52 seconds longer due to experience effects.

Despite the small effect sizes, a likelihood-ratio test shows that the  $SI\ Distance^2$  regressor significantly improves predictions for both lotteries over the simpler model with only  $SI\ Distance$  (P-Bet:  $\chi^2_7=33.72$ ,  $p<0.0001$ ; \$-Bet:  $\chi^2_7=14.40$ ,  $p=0.04$ ). Therefore, we conclude that our data fits in with Moffatt's (2005) more general finding that reaction time increases for choices between options that are more similar in attractiveness.

Table 6.1: Regression Outputs for Choices with P-Bets or \$-Bets against Sure Amounts with log10 of Reaction Time as Dependent Variable

### P-Bet:

Estimated Coefficient	Mean Coefficient Estimate	Median Coefficient Estimate	Std. Error	t-stat	P(>  t )	Wilcoxon Test p-value
Intercept	0.9291	0.9286	0.0019	488.5	<0.001 ***	<0.001 ***
SI Distance	-0.0061	-0.0060	0.0010	-6.623	<0.001 ***	<0.001 ***
SI Distance^2	0.0004	0.0004	0.0001	4.181	<0.001 ***	<0.001 ***
Trial_No^(-1)	0.1243	0.1243	0.0121	10.25	<0.001 ***	<0.001 ***

Individuals: n=74  
Observations: N=1924

Signif. codes: 0 '\*\*\*' 0.001 '\*\*' 0.01 '\*' 0.05 '.' 0.1 ' ' 1

### \$-Bet:

Estimated Coefficient	Mean Coefficient Estimate	Median Coefficient Estimate	Std. Error	t-stat	P(>  t )	Wilcoxon Test p-value
Intercept	0.9238	0.9233	0.0020	451.6	<0.001 ***	<0.001 ***
SI Distance	-0.0014	-0.0013	0.0004	-3.528	0.001 **	0.002 **
SI Distance^2	0.00007	0.00007	0.00003	2.141	0.036 *	0.032 *
Trial_No^(-1)	0.0730	0.0755	0.0052	13.94	<0.001 ***	<0.001 ***

Individuals: n=74  
Observations: N=1924

Signif. codes: 0 '\*\*\*' 0.001 '\*\*' 0.01 '\*' 0.05 '.' 0.1 ' ' 1

(Note that reaction time is measured in log<sub>10</sub> seconds)

## 6.3 Predictions

All BREUT valuation model predictions in this chapter depend on effects due to longer deliberation times. But they can be separated by effects of reaction time on the differences between valuations and SI points, effects on differences between P-Bet and \$-Bet valuations, and effects on the number of observed preference reversals.

### Differences between Valuations and SI Points

As explained in section 6.1.1.1, the BREUT valuation model predicts that longer deliberation times lead to smaller differences between valuations and SI points. Also, the SVM mechanism predicts that candidate values converge towards the SI point the longer a valuation process goes on (as explained in section 6.1.1.2). This simple

proposition is already enough to generate the same testable prediction. Therefore, both models predict that valuations closer to the SI point correlate with individually-longer reaction times. The resulting absolute difference between valuations and respective SI points should therefore decrease for individually-longer reaction times.

However, the BREUT valuation model also predicts that direct valuations fall below the SI point more frequently for longer deliberation times. Even though the average distance between valuations and SI points decreases, fewer valuations will be above and more valuations will be below the SI point. The resulting prediction is that direct valuations are more frequently below the SI point for individually-longer reaction times. Note that all predictions have the same effect direction for the P-Bet as well as the \$-Bet.

### **Differences between P-Bet and \$-Bet Valuations**

Since BREUT obeys weak stochastic transitivity in choices, a choice preference between lotteries also implies a corresponding difference in SI points. The experiment in chapter 5 showed that the majority of participants chooses the P-Bet over the \$-Bet the majority of times. Therefore, if we infer the BREUT preference from these direct choices, the average P-Bet SI point must lie above the average \$-Bet SI point. So a BREUT DM with a choice preference for the P-bet will have a \$-Bet SI point that is already below the P-Bet's SI point. And as explained earlier, average direct valuations of all lotteries decrease towards the respective SI points. The BREUT valuation model also predicts that longer deliberation times reduce \$-Bet valuations more strongly than P-Bet valuations (see section 4.3.3.3). The resulting prediction is that for longer reaction times, the share of \$-Bet valuations above P-Bet valuations decreases because \$-Bet valuations decrease more strongly in the direction of the \$-Bet SI point, which is lower than the P-Bet SI point. So for individually-longer reaction times, BREUT predicts fewer \$-Bet valuations that are higher than P-Bet valuations.

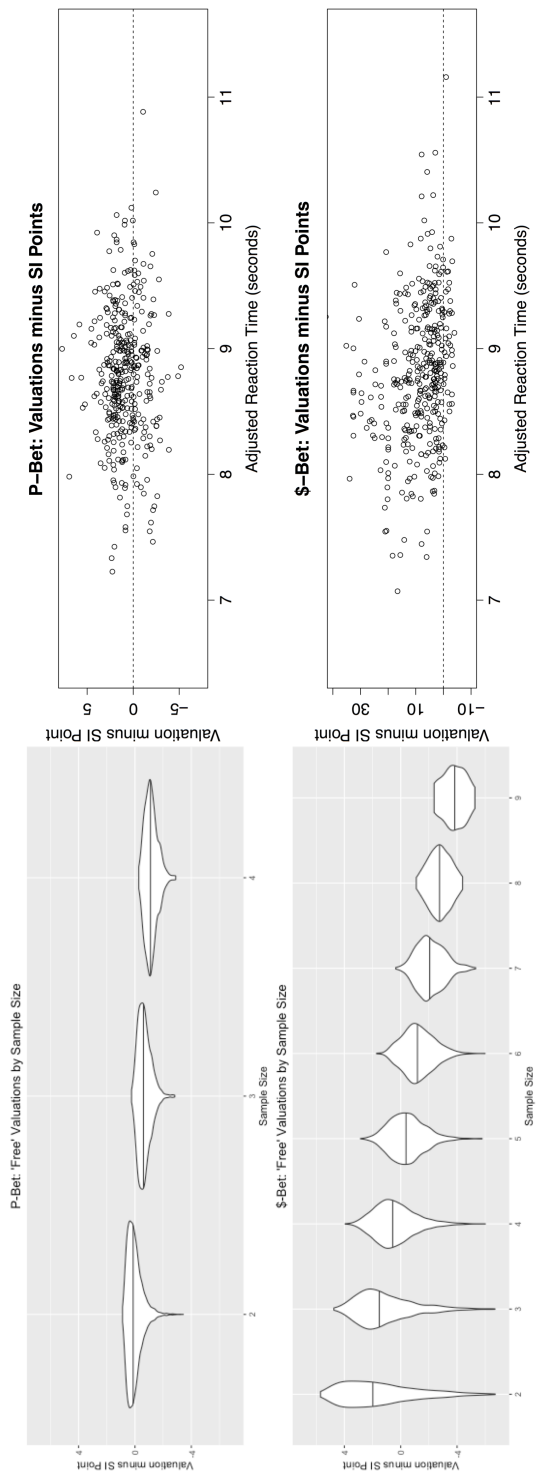
### Preference Reversals: Number of Observed Preference Reversals

If a BREUT valuation DM has a choice preference for the P-Bet, longer deliberation times will lead to more \$-Bet valuations below P-Bet valuations. This implies a preference relationship that is consistent across procedures. Conversely, lottery valuations from shorter deliberation times are more dispersed and further from the respective SI points and result in more preference reversals. The resulting prediction is that for individually-longer reaction times, valuations are more likely to be consistent with preferences in choices. Therefore, the number of observed preference reversals decreases for individually-longer reaction times.

Figure 6.1 shows BREUT valuation model predictions set against scatterplots of differences between valuations and SI points:  $|Valuation_{it} - SI\ Point_i|$ , where  $Valuation_{it}$  is the direct valuation by individual  $i$  in trial  $t$  and  $SI\ Point_i$  their SI point. This is done separately for P-Bets and \$-Bets. Whereas BREUT valuation predictions in the graphs depend on sample sizes, the valuation data depend on reaction times that are adjusted for experience effects. This is done for each participant  $i$  by a simple regression of  $RT_{it} = \beta_{0i} + \beta_{1i}trial_{it}^{-1} + \epsilon_{it}$  where  $trial_{it}^{-1}$  accounts for experience effects (as explained in section 6.2.3.1).  $\beta_{1i}$  has a positive effect on reaction times for early trials, which decreases exponentially throughout the experiment. This reflects longer reaction times due to a lack of experience at the start of the experiment. This effect of  $\beta_{1i}trial_{it}^{-1}$  is then subtracted from individual reaction times  $RT_{it}$  for adjusted reaction times.

Two issues are of interest in figure 6.1. First, as in the BREUT valuation model prediction, average valuations are above the SI point for shorter adjusted reaction times in the scatterplot data and the effect is stronger for the \$-Bet (as shown in section 5.4, chapter 5). Second, there is no clear relationship that shows valuations below the SI point in longer adjusted reaction times, neither for the P-Bet nor for the \$-Bet. Section 6.4 analyses whether the predictions are supported by the data.

Figure 6.1: Effect of Deliberation Time on Valuation SI Point Difference in the BREUT Valuation Model, and Scatterplots of Valuation SI Point Differences against Individual Adjusted Reaction Times





## 6.4 Results

### 6.4.1 Effects of Reaction Time on absolute Differences between Valuations and SI Points

#### Absolute Differences between Valuations and SI Points

To measure individual reaction time effects on absolute differences between valuations and SI points, we fit a simple model separately for P-Bet and \$-Bet valuations and each participant  $i$ :

$$|Valuation_{it} - SI\ Point_i| = \beta_{0i} + \beta_{RTi}RT_{it} + \beta_{2i}Trial\ Index_{it}^{-1} + \varepsilon_{it}$$

for individuals  $i = 1, \dots, 74$  and trials  $t = 1, \dots, 5$ ,

where  $|Valuation_{it} - SI\ Point_i|$  is the absolute difference between the valuation by individual  $i$  in trial  $t$  and individual  $i$ 's SI point,

with an intercept  $\beta_{0i}$ ,

where  $RT_{it}$  is the log10-transformed reaction time of individual  $i$  in trial  $t$ ,

where  $Trial\ Index_{it}^{-1}$  is the inverse of the trial index,

and error term  $\varepsilon_{it}$ .

As explained in the previous section, the prediction is that  $|Valuation - SI\ Point|$  decreases in log-transformed reaction times. So  $H_1$  implies that  $\beta_{RT} < 0$ . This average absolute difference over 5 direct valuation tasks was £1.69 for the P-Bet (median=1.59, sd=1.23) and £9.05 for the \$-Bet (median=6.34, sd=8.13). Table 6.1 shows the regression results for the P-Bet and the \$-Bet. Note that reaction times have been log10-transformed.

For the P-Bet,  $\beta_{RT}$  estimates are positive but the  $\beta_{RT}$  coefficient estimates do not reach significance. So  $H_0$  of no reduction in absolute valuation difference for longer reaction times cannot be rejected. A one-unit increase in log-transformed reaction time statistically increases the absolute valuation difference by £0.17. At the mean P-Bet reaction time of 8.89 seconds, this equates to only a £0.002 increase in absolute valuation difference for a 1 second increase in reaction time.

For the \$-Bet,  $\beta_{RT}$  estimates are negative and reach significance. A likelihood-

ratio test shows that the  $RT$  regressor statistically significantly improves predictions over a simpler model without it ( $\chi^2_1=9.27$ ,  $p<0.001$ ). A one-unit increase in log-transformed reaction time statistically decreases absolute valuation difference by £14.79. At the mean \$-Bet reaction time of 9.134 seconds, this equates to a £0.18 decrease in absolute valuation difference for a 1 second increase in reaction time. Even though this effect is significant, theoretically a 1 second increase in reaction time only reduces the mean difference by 2%. But average \$-Bet valuations are £8.71 above the SI point. Therefore, this effect is unlikely to result in any valuations below the SI point due to longer reaction times. So while the regression for the \$-Bet rejects  $H_0$  and shows a reduction of absolute valuation difference in line with the predictions, the effect size appears to be too small to compensate the preference reversal in longer reaction times.

Table 6.2: Regression Output of Individual Effects on Absolute Difference between Valuation and SI point

### P-Bet:

Estimated Coefficient	Mean Coefficient Estimate	Median Coefficient Estimate	Std. Error	t-stat	P(>  t )	Wilcoxon Test p-value
Intercept	1.5132	1.8640	0.4349	3.480	<0.001 ***	<0.001 ***
log_RT	0.1684	-0.3346	0.4721	0.357	0.722	0.323
Trial_No <sup>(-1)</sup>	0.9351	1.1480	0.1137	8.2252	<0.001 ***	<0.001 ***
Individuals: n=74 Observations: N=370						
Signif. codes: 0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1						

### \$-Bet:

Estimated Coefficient	Mean Coefficient Estimate	Median Coefficient Estimate	Std. Error	t-stat	P(>  t )	Wilcoxon Test p-value
Intercept	22.685	19.560	1.4367	15.79	<0.001 ***	<0.001 ***
log_RT	-14.788	-13.420	0.6303	-23.462	<0.001 ***	<0.001 ***
Trial_No <sup>(-1)</sup>	40.385	47.283	2.7501	14.685	<0.001 ***	<0.001 ***
Individuals: n=74 Observations: N=370						
Signif. codes: 0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1						

(Note that reaction time is measured in log<sub>10</sub> seconds)

## Effects of Reaction Time on the Difference between Valuation and SI Point

The BREUT valuation model predicts that individually-longer reaction times result in valuations that are more frequently below the SI point. On a qualitative level, we can test for this by observing the share of direct valuations  $CE_{DV}$  below the SI point  $CE_{SI}$  for shorter reaction times versus this share for longer reaction times.

Separately for P-Bets and \$-Bets, we adjust reaction times to experience effects as described in section 6.3. Then, we assign a rank to each reaction time on a per-individual basis, with rank 1 for the shortest and rank 5 for the longest adjusted reaction time. That way, we separate elicited valuations into 5 groups according to the length of the reaction time on an individual basis. For each group, we can then observe how many  $CE_{DV}$  valuations were above, at, or below the SI point  $CE_{SI}$ .

Table 6.3 shows this cross-tabulation for P-Bet valuations. In total, 261 P-Bet valuations were above the respective participant's SI point and 107 below (with another 2 valuations exactly at the SI point). But from the shortest individual reaction time (rank 1) to the longest reaction time (rank 5), the number of valuations below the SI point even decreases. This reflects the positive effect of individually-longer reaction times, which was reported in the regression of reaction times on the absolute difference between valuations and SI points. Therefore,  $H_0$  cannot be rejected.

We can test if this increase in P-Bet valuations above the SI point is statistically significant. As rank categories are ordinal, we need to test our hypotheses with an ordinal  $\chi^2$  test<sup>1</sup>, which is more likely to correctly reject  $H_0$  (Agresti, 2013). This test reports no statistically significant effect<sup>2</sup> ( $\chi^2_2=0.70$ ,  $p=0.703$ ).

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<sup>1</sup>Agresti (2013) also refers to the special case of one ordinal variable and another non-ordinal variable with two categories as “extended Cochran–Armitage test”. The general  $m \times n$  version of this (with  $m, n \in \mathbf{N}$ ) is referred to as a “linear-by-linear association model” Agresti (2013).

<sup>2</sup>This result also holds when the column with P-Bet valuations equal to the SI point is removed (Cochran–Armitage test:  $z^2=0.654$ ,  $p=0.513$ ).

Table 6.3: **P-Bet**: Cross Tabulation of Reaction Time Ranks and Differences between Direct Valuations and SI Points

Difference between participants' $CE_{DV}$ Values and $CE_{SI}$ Values					
Reaction Time Rank (1-shortest, 5-longest)		$CE_{DV}>CE_{SI}$	$CE_{DV}=CE_{SI}$	$CE_{DV}<CE_{SI}$	count
	1	52	0	22	<b>74</b>
	2	50	1	23	<b>74</b>
	3	50	1	23	<b>74</b>
	4	55	0	19	<b>74</b>
	5	54	0	20	<b>74</b>
	count	<b>261</b>	<b>2</b>	<b>107</b>	<b>370</b>

Table 6.4 shows the same cross-tabulation for \$-Bet valuations. In total, 334 \$-Bet valuations were above the SI point and 35 below (with 1 valuation exactly at the SI point). Despite the negative effect of individually-longer reaction times on absolute differences between valuations and SI point, valuations above the SI point only decrease from 69 to 67 from rank 1 to 5. An ordinal  $\chi^2$  test reports no statistically significant effect<sup>3</sup> ( $\chi^2_2=2.01$ ,  $p=0.367$ ) and  $H_0$  cannot be rejected.

Therefore, there is no evidence for the BREUT valuation model's prediction that the number of valuations above SI points decreases in individually-longer reaction times, neither for P-Bets nor for \$-Bets. In addition, the decrease in \$-Bet valuations is not sufficiently strong to result in a statistically significant reduction of preference reversals (when comparing direct valuations to SI points).

<sup>3</sup>This result also holds when the column with \$-Bet valuations equal to the SI point is removed (Cochran–Armitage test:  $z^2=0.024$ ,  $p=0.981$ ).

Table 6.4: **\$-Bet**: Cross Tabulation of Reaction Time Ranks and Differences between Direct Valuations and SI Points

Difference between participants' $CE_{DV}$ Values and $CE_{SI}$ Values					
Reaction Time Rank (1-shortest, 5-longest)		$CE_{DV}>CE_{SI}$	$CE_{DV}=CE_{SI}$	$CE_{DV}<CE_{SI}$	count
	1	69	0	5	<b>74</b>
	2	65	0	9	<b>74</b>
	3	66	0	8	<b>74</b>
	4	67	0	7	<b>74</b>
	5	67	1	6	<b>74</b>
	count	<b>334</b>	<b>1</b>	<b>35</b>	<b>370</b>

#### 6.4.2 Effects of Reaction Time on Differences between P-Bet and \$-Bet valuations

An additional prediction of the BREUT valuation model is that for longer reaction times, the stronger reduction in \$-Bet valuations results in more \$-Bet valuations below P-Bet valuations of the same individual. We can test for this with a similar cross tabulation as in the last section. But this time, we match P-Bet valuations ( $CE_{DV}(P)$ ) to \$-Bet valuations ( $CE_{DV}(\$)$ ) of the same rank and individual. Thereby, we count at the individual level how many P-Bet valuations were above, equal to, or below \$-Bet valuations of the same adjusted reaction time rank.

The corresponding prediction is that we observe more P-Bet valuations above \$-Bet valuations ( $CE_{DV}(P) > CE_{DV}(\$)$ ) for higher reaction time ranks. Table 6.5 lists the results. But again, we actually observe a weak opposite effect: Higher \$-Bet valuations occur 59 out of 74 times for shortest reaction times and 60 out of 74 times for longest reaction times. And an ordinal  $\chi^2$  test reports no statistically significant effect<sup>4</sup> ( $\chi^2=4.07$ ,  $p=0.131$ ).

Section 6.4.1 showed that in fact, individually-longer reaction times have a weak positive effect on P-Bet valuations and a negative effect on \$-Bet valuations.

<sup>4</sup>This result also holds when the column with equal valuations is removed (Cochran–Armitage test:  $z^2=0.651$ ,  $p=0.513$ ).

But despite this, these combined effects appear to be insufficient to lead to fewer \$-Bet valuations above P-Bet valuations for higher adjusted reaction times. Therefore again,  $H_0$  cannot be rejected.

Table 6.5: Cross Tabulation of Reaction Time Ranks and Differences between Direct P-Bet and \$-Bet Valuations

Reaction Time Rank (1-shortest, 5-longest)	Difference between participants' $CE_{DV}$ Values for P-Bet and \$-Bet			
	$CE_{DV}(P) > CE_{DV}(\$)$	$CE_{DV}(P) = CE_{DV}(\$)$	$CE_{DV}(P) < CE_{DV}(\$)$	count
1	15	0	59	<b>74</b>
2	11	2	61	<b>74</b>
3	13	1	60	<b>74</b>
4	13	2	59	<b>74</b>
5	10	4	60	<b>74</b>
count	<b>62</b>	<b>9</b>	<b>299</b>	<b>370</b>

#### 6.4.3 Effects of Reaction Time on the Number of Observed Preference Reversals

In a final step, we can also test a prediction of the BREUT valuation model that is also shared with the SVM mechanism: that two lottery valuations are more likely to reflect a choice preference for longer reaction times. I.e., individually-longer valuation processes are more likely to produce CEs that are consistent with the participant's average choice preference between lotteries. Thereby, the number of observed preference reversals decreases for individually-longer reaction times.

In order to analyse this, we have to separate between 4 possible cases per reaction time rank: 2 instances where preferences are consistent and 2 instances where preferences are inconsistent:

1. Prefer P over \$ and value P higher than \$: Consistent Preference
2. Prefer P over \$ but value \$ higher than P: Preference Reversal
3. Prefer \$ over P and value \$ higher than P: Consistent Preference

#### 4. Prefer \$ over P but value P higher than \$: Preference Reversal

Table 6.6 lists matched valuations from the same participant and adjusted reaction time rank according to these characteristics. Choice preference is defined as the majority direct choice between P-Bet and \$-Bet across the five repetitions in the experiment. The preference in valuations results from comparing the P-Bet and \$-Bet valuation of the same adjusted reaction time rank from the same participant. Table 6.6 lists 199 observations of the classic preference reversal, where the P-Bet is chosen more often ( $P \succ \$$ ) but \$-Bet valuations are higher than or equal to P-Bet valuations ( $CE_{DV}(P) < CE_{DV}(\$)$ ). The table also lists 170 observations of consistent preferences, 61 for the P-Bet and 109 for the \$-Bet.

The prediction is that the number of preference reversals will decrease for higher reaction time ranks. But the number of classic preference reversals actually increases from 37 at rank 1 to 43 at rank 5. An ordinal  $\chi^2$  test reports no statistically significant effect<sup>5</sup> ( $\chi^2_3=3.09$ ,  $p=0.378$ ) and  $H_0$  cannot be rejected.

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<sup>5</sup>This result also holds when we consolidate the columns so that one column lists valuation preferences that match with choice preferences and a second column lists valuation preferences that do not match with choice preferences. I.e., when we only compare the number of preference reversals to the number of consistent preference relations per reaction time rank (Cochran–Armitage test:  $z^2=0.885$ ,  $p=0.376$ ).

Reaction Time Rank  
(1-shortest, 5-longest)

Table 6.6: Cross Tabulation of Reaction Time Ranks and Differences between Direct P-Bet and \$-Bet Valuations

Difference between participants' $CE_{DV}$ Values for P-Bet and \$-Bet					
	1. Majority $P \succ \$$ and $CE_{DV}(P) > CE_{DV}(\$)$	2. Majority $P \succ \$$ and $CE_{DV}(P) \leq CE_{DV}(\$)$	3. Majority $P \prec \$$ and $CE_{DV}(P) < CE_{DV}(\$)$	4. Majority $P \prec \$$ and $CE_{DV}(P) \geq CE_{DV}(\$)$	count
1	15	37	22	0	74
2	11	41	22	0	74
3	13	39	22	0	74
4	13	39	22	0	74
5	9	43	21	1	74
count	61	199	109	1	370



## 6.5 Contribution and Discussion

A large body of literature exists on sequential sampling models, which assumes that reaction times in experiments do at least partly reflect the extent to which participants deliberate for a decision. The computational models that are discussed in chapter 3 and the BREUT valuation model developed in chapter 4 also reflect the assumption that “more sampling is more precise”: In theory, a longer deliberation time leads to valuations that are closer to individuals’ preferences as represented by their underlying core functions. These underlying preferences are assumed to be more consistent with individuals’ choices than with their valuations.

This rough criterion predicts that discrepancies between valuations and choices decrease in individually-longer reaction times. We repeatedly tested for evidence that differences between valuations and SI points decrease in individually-longer reaction times in direct valuation tasks with three different approaches: 1) By measuring the absolute difference between direct valuations and SI points as well as the frequency of valuations above SI points, 2) by measuring the frequencies of implied preference relations between P-Bet and \$-Bet valuations with the same adjusted reaction time rank, and 3) by measuring the frequency of observed preference reversals per adjusted reaction time rank.

Note that repeated testing also increases the likelihood of detecting the predicted effect on at least one occasion. But in all cases, there was no sufficient evidence that preference reversals decrease in reaction time at the individual level. Section A.4 in the Appendix also shows that outlier removal does not alter results.

The phenomenon that reaction times reflect a speed-accuracy trade-off in decision making has been documented across a range of decision tasks. This makes reaction times as an explanatory factor of the preference reversal phenomenon especially interesting. The BREUT valuation model, SP model, and SVM mechanism predict that too high lottery valuations will decrease throughout a valuation process and converge towards a value that is more likely to be consistent with participants’ preferences in choices. But our analysis suggests that this is not the case when we compare valuations to individual SI points and choice data. Therefore, this evidence

shows that a different approach is necessary to adequately capture how discrepancies between valuations, SI points, and choice behaviour arise and when they are reduced.

## Chapter 7

# Conclusion

This thesis reviewed existing research applicable to the preference reversal phenomenon and the MSoP mismatch with a focus on bounded rationality. Existing research only produced sequential sampling models of valuation that use a “choose and adjust” approach, which does not predict lottery overvaluations relative to SI points as observed by Loomes and Pogrebna (2016).

It was possible to extend the BREUT choice model to also predict valuations and MSoP values. The BREUT valuation model samples from a distribution of mental evidence, which generates CE values from utility functions that satisfy the conventional economic concept of rationality. Even though it fully predicts choices and valuations by sampling from the same distribution of mental evidence, it systematically predicts the preference reversal phenomenon. The model is stochastically transitive but violates procedure invariance: Lotteries tend to be overvalued relative to their SI points. This tendency is stronger for \$-Bets than for P-Bets, leading to systematic preference reversals. The valuation model can also predict a positive MSoP mismatch by incorporating spill-over effects from choice onto valuations. In addition, it is possible to impose effects from consistency-seeking behaviour on the model, which can provide an added positive effect to the MSoP mismatch.

We conducted an experiment that estimates CEs through different elicitation methods. This quantifies the extent of the preference reversal and MSoP mismatches in relation to individual SI points. Measuring SI points also allowed us

to test whether anchoring effects as specified by the SVM mechanism and the SP model can explain preference reversals. In addition, the experiment also controlled for the delay between choices and MSoP elicitations, thereby distinguishing between spill-over effects and consistency-seeking behaviour. The experiment shows that lotteries are overvalued relative to their SI points, contrary to the “choose and adjust” explanation via anchoring effects. This effect is stronger for the \$-Bet, leading to the preference reversal phenomenon. The positive MSoP mismatch is replicated when participants report MSoP values through upward adjustments of sure amounts. But a previously unknown negative MSoP mismatch emerges for downward adjustments. There is no difference in MSoP mismatches between immediate and delayed adjustments, suggesting that spill-over effects are not causal to the MSoP mismatch. However, consistency-seeking behaviour alone cannot explain a negative MSoP mismatch either.

We also tested for effects of individually-longer reaction times on direct valuations. But although the distance between valuations and SI points decreases for individually-longer reaction times for \$-Bets, this effect is not strong enough to have an effect on observed preference reversals. This result also applies to reversals between valuations and SI points, between P-Bet and \$-Bet valuations, and to reversals from choices to valuations.

An interesting future direction of research would be to explore if other extensions of the BREUT model as well as other sequential sampling models can produce predictions of valuations that are entirely based on sampling. While the BREUT valuation model does predict preference reversals, it underpredicts the strong effects frequently reported in the literature. The phenomenon that longer deliberation times might not lead to fewer preference reversals also poses a challenge to approaches based on sequential sampling. There is no sequential sampling model using a core utility function that can predict all results from the experiment. CE elicitations that are affected by the procedure pose a fundamental problem at the basis of economic research. For economic theory to accurately capture behaviour, valuation models need to be improved to explain the source of these inconsistencies.

The valuation experiment was also designed to minimise the risk of endowment effects that might influence MSoP tasks. And MSoP values did not differ when the MSoP elicitation was delayed after the choice, suggesting that no such effect occurred. But an interesting extension would be to check how the MSoP phenomenon could relate to an endowment effect. E.g., MSoP values could be elicited after choices between non-monetary alternatives. And instead of classic WTA and WTP elicitations, participants could be asked for the MSoP equivalent. Depending on participants actually obtaining a good, an MSoP elicitation method might generate mismatches with choices or WTA/WTP elicitations independent of any endowment effect or modulate its strength.

The reaction times analysis suggests that longer deliberation times do not lead participants to adjust their valuations in a way that they become more consistent with their preference in choices. But the sequential sampling models discussed in this thesis do predict such an effect. In a first step to inform theory, it could be useful to check if participants in valuation experiments are generally hesitant to adjust *any* money amount downwards. Building a model that accounts for this behaviour might provide a better approach than a DM updating a sample mean as used in the BREUT valuation model.

While behavioural regularities that violate EUT are well-documented, as-if models such as the one developed in chapter 4 are far from catching up in predicting these regularities. Studying actual mechanisms in the human brain might lead modelling efforts in the proper direction. A number of brain regions have been successfully identified to be linked to mental processes that are used for utility optimisation processes (Louie et al., 2015). These processes depend on a neural representation of value information, where a choice is influenced by a process more akin to perception instead of a construction of preference. E.g., where a choice depends on identifying an option with a higher value instead of relying on a notion of preference, which is fundamental to completing preference elicitation tasks. As these process are subject to the constraints of the human brain, this again mirrors the satisficing condition of bounded rationality.

These processes are also modulated by context effects because valuation behaviour appears to be comparative instead of absolute, e.g. leading to loss aversion, violations of the independence axiom, and effects of expectations on reference points that can also be shaped by learning (Louie et al., 2013; Tymula and Plassmann, 2016). Tymula and Glimcher (2018) develop a model to more accurately represent neurological processes in the human brain. The model has a novel functional form that incorporates cardinal instead of ordinal representations of utility and a bounded and finite value function with limited precision, which can adjust to changes in a reference point. Different preference estimations across participants arise due to heterogeneity in their parameters for calculating value, not because of different estimations of preference. Also, previous experiences shape behaviour through their influence on reference points. But this leads back to the initial problem in a straight valuation task: How will a model describe a lottery valuation when there is no other alternative to compare the lottery to? This will require additional research to explore how such models might overcome this problem in predicting choice-matching discrepancies.

In conclusion, there is both a lot of evidence and a lot of theoretical work on valuation behaviour. But there is still no model using a core utility function that can reconcile all phenomena observed in the experiment, let alone for choice-matching discrepancies in general. If existing theory is already limited in capturing these results, predictions in more complicated contexts could be affected as well. So further theoretical work on building models that can explain these phenomena may have implications for economic theory in general.

# Appendix A

## A.1 Chapter 3 and 4: Certainty Equivalents of P-Bet and \$-Bet dependent on $r$

Consider the following utility function with random risk-aversion parameter  $r$ :

$$u(x) = x^{1-r} \quad \text{where } r \text{ is randomly distributed}$$

See figure A.1 for such a CE distribution: With  $r \sim \text{uniform}[-0.3; 0.7]$ , a mean risk-averse DM is facing a 80% chance of £12 (P-Bet) vs. a 25% chance of £50 (\$-Bet). Note that some CE values are higher than the lotteries' expected values as the minority of  $r$  parameters below zero generate risk-seeking CEs. Even though the uniform distribution is symmetric, the resulting distribution of CE values is not.

See figure A.2 for a graph how CE values of the P-Bet and the \$-Bet depend on  $r$  and the exponent in  $u(x) = x^{1-r} = x^s$  where  $s = (1 - r)$ . A change in  $r$  results in a non-linear change in the CE values of the lotteries.

Figure A.1: CE distribution of a “mean risk-averse” DM with a uniform distribution of risk aversion parameters with  $r \in [-0.3; 0.7]$

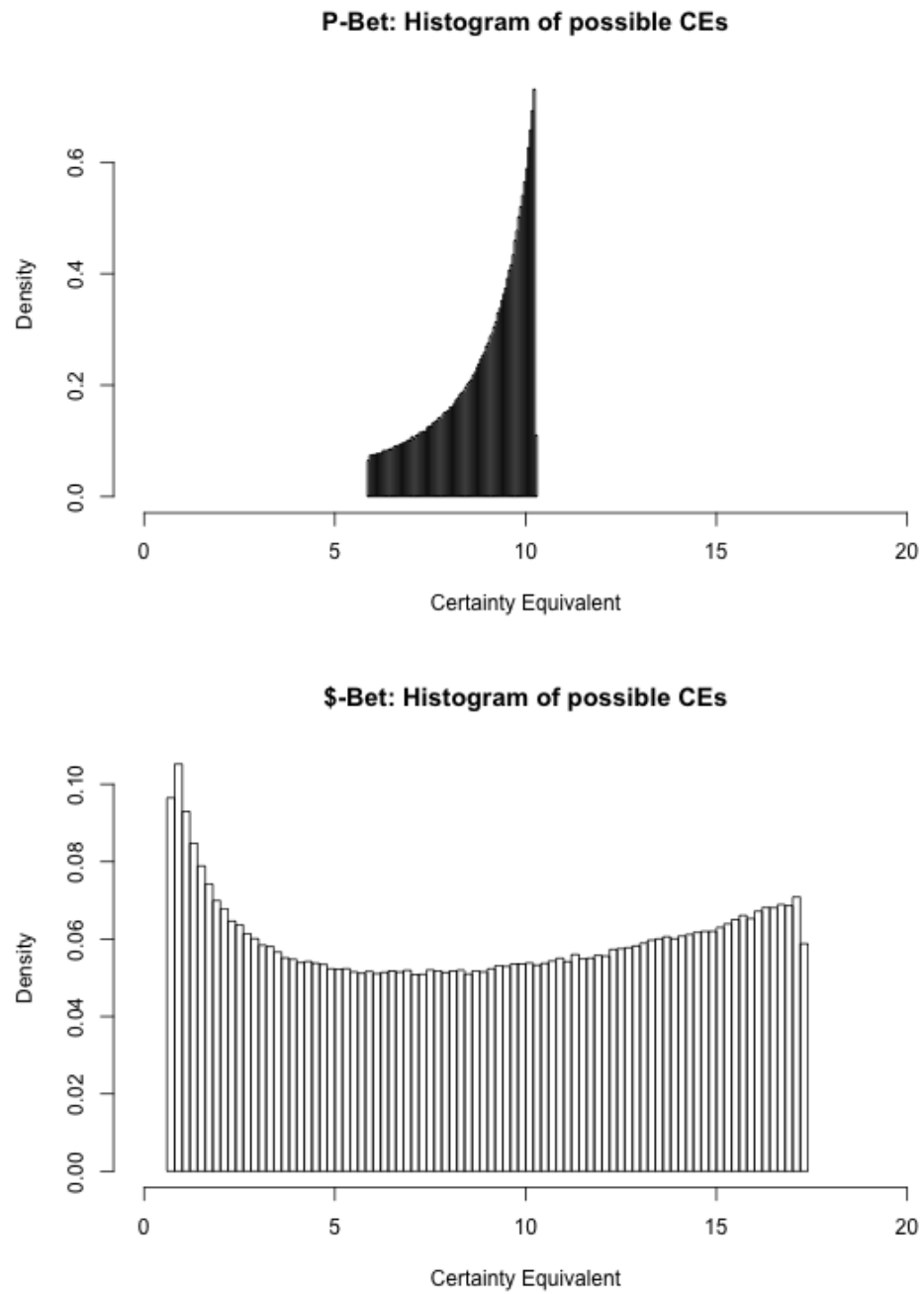
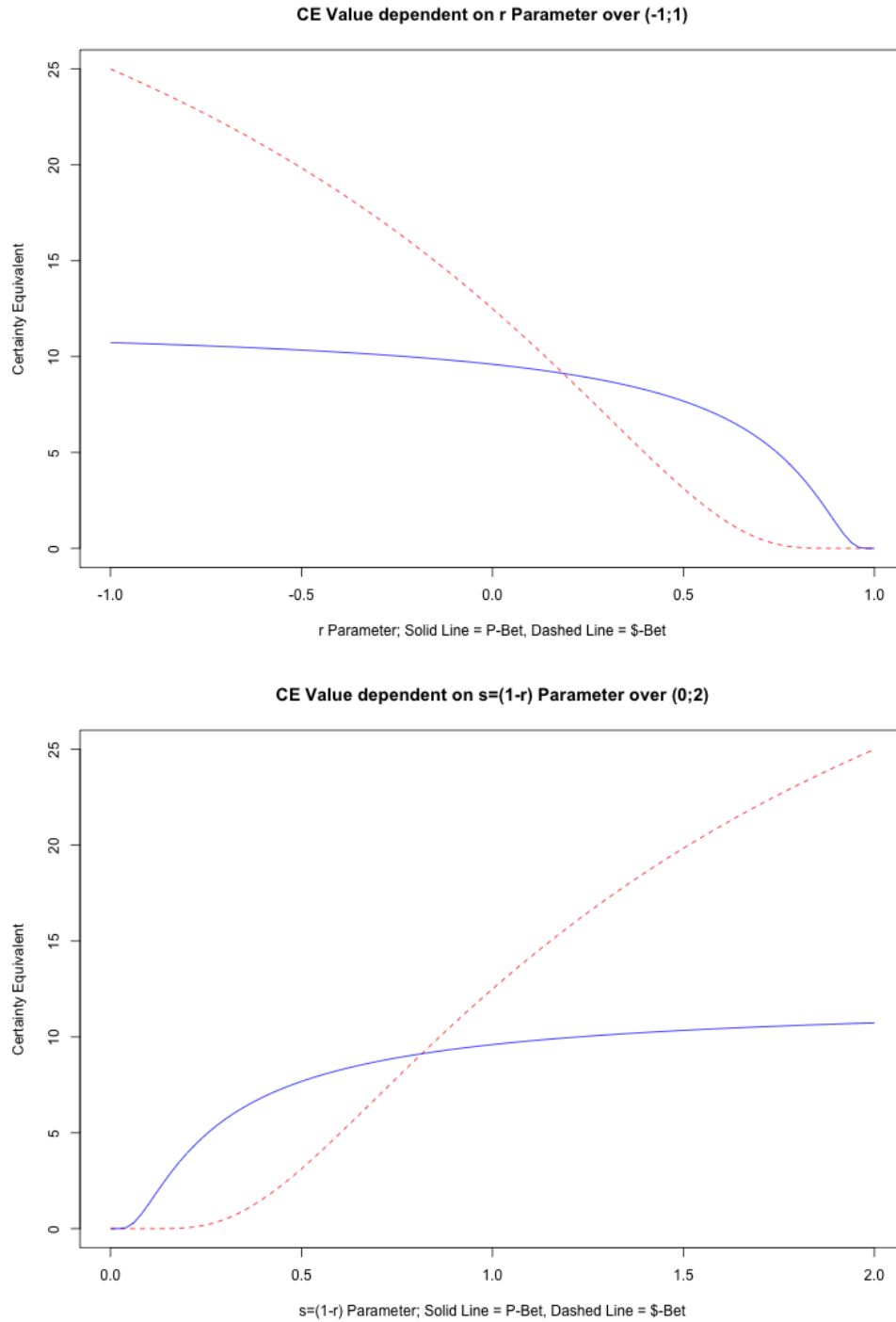




Figure A.2: CE values of the utility function  $u(x) = x^{1-r}$  dependent on the risk aversion parameter  $r \in (-1; 1)$  and its result  $s = (1 - r) \in (0; 2)$



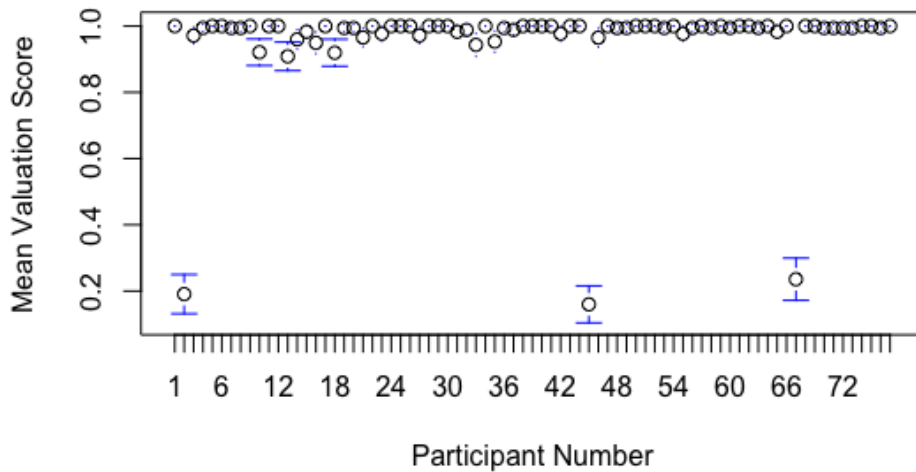
Note: The interval  $(-1; 1)$  for  $r$  excludes  $r = 1$ , therefore  $CE(s = 0)$  is not plotted

## A.2 Chapter 5 and 6: Data Exclusion

For each participant, a score was calculated for each task involving a valuation via slider (*IA*, *DV*, and *LA* Tasks). This score was 0 in case of the valuation being higher than 95% or lower than 5% of the winning payoff of the lottery, 1 otherwise. A participant's mean score across the entire experiment corresponds to the percentage of tasks in which they stated a preference that was not particularly extreme as only outliers generate a score of 0. As soon as a participant's mean score is below 0.5, their data would be excluded.

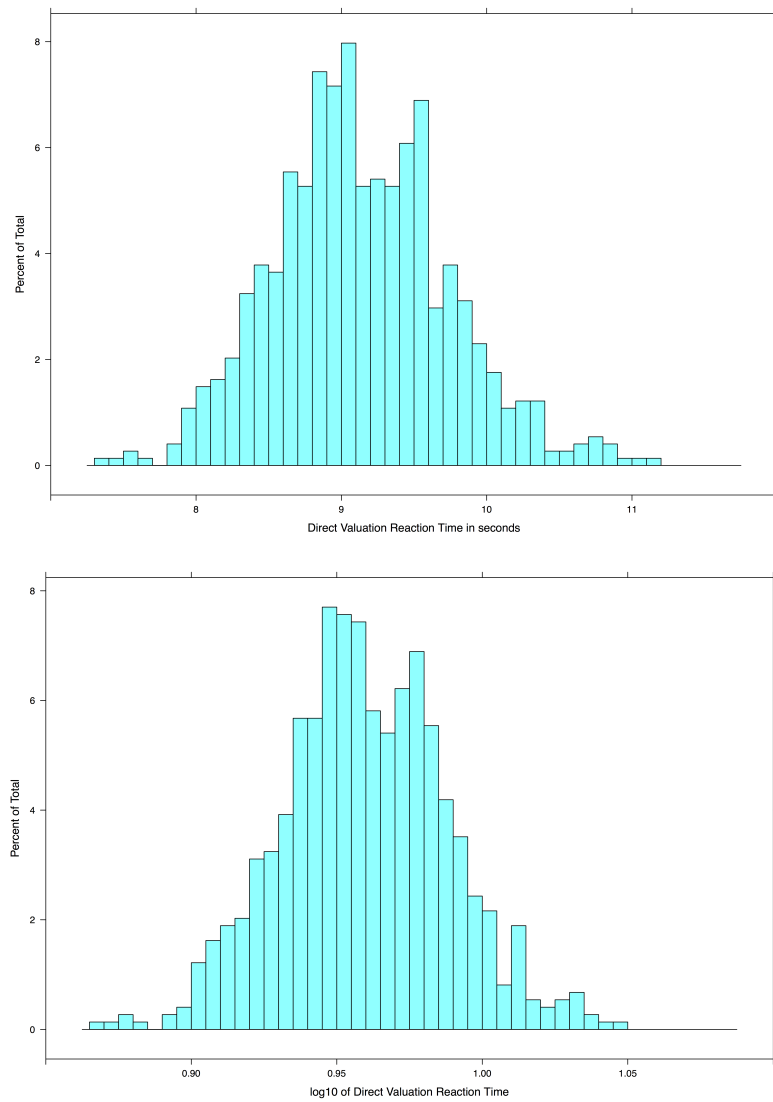
Mean valuation scores for each participant were calculated and are shown in figure A.3. Three participants with a mean valuation score below 0.25 were excluded from the analysis to avoid using data from participants that did not understand the BDM procedure. After excluding these participants, the remaining sample had a collective valuation score of 0.99 (min=0.91; max=1).

Figure A.3: Mean Valuation Scores for all participants



### A.3 Chapter 6: Figures

Figure A.4: Histograms of Reaction Times and  $\log_{10}$  transformed Reaction Times of MSoP tasks



### A.4 Chapter 6: Outlier Removal

In addition to the main lotteries, the data also covers a range of lotteries that were used as distractor tasks (in a randomised order within distractors). Participants were

randomly assigned to two different groups with different distractor tasks. Table A.1 details the lotteries. The main P-Bet and \$-Bet were the same for group A (38 participants) and group B (36 participants). Distractor lotteries are divided into P-type lotteries with winning probabilities above 50% and \$-type lotteries with winning probabilities below 50%.

Separately for P-type and \$-type lotteries, mean adjusted reaction times were computed as described in chapter 6 (section 6.2). Following a method described by Ratcliff (1993), outliers were removed by computing the mean adjusted reaction time as well as the corresponding standard deviation for each individual across all direct valuation tasks. Note that including distractor lotteries reduces the standard deviation because a larger sample is used. This makes the outlier removal method more conservative.

All valuation tasks where participants took longer than their mean adjusted reaction time plus two standard deviations to respond, were designated as outliers and removed. This occurred for 17 P-Bet tasks and 14 \$-Bet tasks. There was no participant for whom more than 1 valuation task was removed per P-Bet or \$-Bet lottery. The following tables list the results from chapter 6 with removed outliers:

- Table A.2: Individual Effects of Reaction Time on Overvaluation
  - For the P-Bet, the  $RT$  regressor does not statistically significantly improve predictions over a simpler model without it (likelihood-ratio test:  $\chi^2=0.01$ ,  $p=0.944$ )
  - For the \$-Bet, the  $RT$  regressor statistically significantly improves predictions over a simpler model without it (likelihood-ratio test:  $\chi^2=34.66$ ,  $p<0.001$ )
- Table A.3: Individual Effects of Reaction Time on the Difference between P-Bet Valuation and SI Point
  - An ordinal  $\chi^2$  test reports no statistically significant effect ( $\chi^2_2=0.983$ ,  $p=0.612$ )

- This result also holds when the column with P-Bet valuations equal to the SI point is removed (Cochran–Armitage test:  $z^2=0.898$ ,  $p=0.369$ )
- Table A.4: Individual Effects of Reaction Time on the Difference between \$-Bet Valuation and SI Point
  - An ordinal  $\chi^2$  test reports no statistically significant effect ( $\chi^2_2=2.457$ ,  $p=0.293$ )
  - This result also holds when the column with P-Bet valuations equal to the SI point is removed (Cochran–Armitage test:  $z^2=0.442$ ,  $p=0.658$ )
- Table A.5: Individual Effects of Reaction Time on Differences between P-Bet and \$-Bet valuations
  - An ordinal  $\chi^2$  test reports no statistically significant effect ( $\chi^2_2=3.185$ ,  $p=0.203$ )
  - This result also holds when the column with equal valuations is removed (Cochran–Armitage test:  $z^2=0.301$ ,  $p=0.764$ )
- Table A.6: Individual Effects of Reaction Time on the Number of Observed Preference Reversals
  - An ordinal  $\chi^2$  test reports no statistically significant effect ( $\chi^2_3=2.934$ ,  $p=0.402$ )
  - This result also holds when comparing the number of preference reversals to the number of consistent preference relations per reaction time rank (Cochran–Armitage test:  $z^2=0.799$ ,  $p=0.424$ )

Table A.1: Lotteries that were displayed to Participants for Direct Valuation Tasks

<b>Group A</b> (38 participants)	<b>Lottery</b>				# of Repetitions
	Payoff 1	Prob. 1	Payoff 2	Prob. 2	
P-Bet	£0	0.20	£12	0.80	5
P-type Distractor	£0	0.10	£15	0.90	2
P-type Distractor	£0	0.20	£15	0.80	2
P-type Distractor	£0	0.30	£15	0.70	2
\$-Bet	£0	0.75	£50	0.25	5
\$-type Distractor	£0	0.80	£40	0.20	2
\$-type Distractor	£0	0.75	£40	0.25	2
\$-type Distractor	£0	0.70	£40	0.30	2

<b>Group B</b> (36 participants)	<b>Lottery</b>				# of Repetitions
	Payoff 1	Prob. 1	Payoff 2	Prob. 2	
P-Bet	£0	0.20	£12	0.80	5
P-type Lottery	£0	0.10	£14	0.90	2
P-type Lottery	£0	0.20	£14	0.80	2
P-type Lottery	£0	0.30	£14	0.70	2
\$-Bet	£0	0.75	£50	0.25	5
\$-type Lottery	£0	0.80	£60	0.20	2
\$-type Lottery	£0	0.75	£60	0.25	2
\$-type Lottery	£0	0.70	£60	0.30	2

Table A.2: Regression Output of log10 Reaction Time Effects on Overvaluation

**P-Bet:**

Estimated Coefficient	Mean Coefficient Estimate	Median Coefficient Estimate	Std. Error	t-stat	P(> t )	Wilcoxon Test p-value
Intercept	1.5132	1.8640	0.4349	3.480	<0.001 ***	<0.001 ***
log_RT	0.1684	-0.3346	0.4721	0.357	0.722	0.323
Trial_No^(-1)	0.9351	1.1480	0.1137	8.2252	<0.001 ***	<0.001 ***
Individuals: n=74 Observations: N=370						
Signif. codes: 0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1						

**\$-Bet:**

Estimated Coefficient	Mean Coefficient Estimate	Median Coefficient Estimate	Std. Error	t-stat	P(> t )	Wilcoxon Test p-value
Intercept	22.685	19.560	1.4367	15.79	<0.001 ***	<0.001 ***
log_RT	-14.788	-13.420	0.6303	-23.462	<0.001 ***	<0.001 ***
Trial_No^(-1)	40.385	47.283	2.7501	14.685	<0.001 ***	<0.001 ***
Individuals: n=74 Observations: N=370						
Signif. codes: 0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1						

(Note that reaction time is measured in log<sub>10</sub> seconds)

Table A.3: **P-Bet:** Cross Tabulation of Reaction Time Ranks and Differences between Direct Valuations and SI Points

		Difference between participants' $CE_{DV}$ Values and $CE_{SI}$ Values			
		$CE_{DV} > CE_{SI}$	$CE_{DV} = CE_{SI}$	$CE_{DV} < CE_{SI}$	count
Reaction Time Rank (1-shortest, 5-longest)	1	52	0	22	<b>74</b>
	2	50	1	23	<b>74</b>
	3	50	1	23	<b>74</b>
	4	55	0	19	<b>74</b>
	5	43	0	14	<b>57</b>
count		<b>250</b>	<b>2</b>	<b>101</b>	<b>353</b>

Table A.4: **\$-Bet**: Cross Tabulation of Reaction Time Ranks and Differences between Direct Valuations and SI Points

Difference between participants' $CE_{DV}$ Values and $CE_{SI}$ Values					
Reaction Time Rank (1-shortest, 5-longest)		$CE_{DV}>CE_{SI}$	$CE_{DV}=CE_{SI}$	$CE_{DV}<CE_{SI}$	count
	1	69	0	5	<b>74</b>
	2	65	0	9	<b>74</b>
	3	66	0	8	<b>74</b>
	4	67	0	7	<b>74</b>
	5	56	1	3	<b>60</b>
	count	<b>323</b>	<b>1</b>	<b>32</b>	<b>356</b>

Table A.5: Cross Tabulation of Reaction Time Ranks and Differences between Direct P-Bet and \$-Bet Valuations

Difference between participants' $CE_{DV}$ Values for P-Bet and \$-Bet					
Reaction Time Rank (1-shortest, 5-longest)		$CE_{DV}(P) > CE_{DV}(\$)$	$CE_{DV}(P) = CE_{DV}(\$)$	$CE_{DV}(P) < CE_{DV}(\$)$	count
	1	15	0	59	<b>74</b>
	2	11	2	61	<b>74</b>
	3	13	1	60	<b>74</b>
	4	13	2	59	<b>74</b>
	5	9	3	45	<b>57</b>
	count	<b>61</b>	<b>8</b>	<b>284</b>	<b>353</b>



Table A.6: Cross Tabulation of Reaction Time Ranks and Differences between Direct P-Bet and \$-Bet Valuations

Reaction Time Rank (1-shortest, 5-longest)	Difference between participants' $CE_{DV}$ Values for P-Bet and \$-Bet				count
	1. Majority $P \succ \$$ and $CE_{DV}(P) > CE_{DV}(\$)$	2. Majority $P \succ \$$ and $CE_{DV}(P) \leq CE_{DV}(\$)$	3. Majority $P \prec \$$ and $CE_{DV}(P) < CE_{DV}(\$)$	4. Majority $P \prec \$$ and $CE_{DV}(P) \geq CE_{DV}(\$)$	
	15	37	22	0	
	11	41	22	0	
	13	39	22	0	
	13	39	22	0	
5	8	33	15	1	57
count	60	189	103	1	353

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